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Office of publication: Curtis Reed Plaza, Menasha, Wisconsin.
Foreign Agent: P. S. King and Staples, Ltd., Great Smith Street, Westminster, London.
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The American Political Science Review

VOL. LXIV DECEMBER, 1970 NO. 4

CONCEPT MISFORMATION IN COMPARATIVE POLITICS*

GIOVANNI SARTORI
University of Florence

"To have mastered 'theory' and 'method' is to have become a *conscious thinker*, a man at work and aware of the assumptions and implications of whatever he is about. To be mastered by 'method' or 'theory' is simply to be kept from working."[1] The sentence applies nicely to the present plight of political science. The profession as a whole oscillates between two unsound extremes. At the one end a large majority of political scientists qualify as pure and simple unconscious thinkers. At the other end a sophisticated minority qualify as overconscious thinkers, in the sense that their standards of method and theory are drawn from the physical, "paradigmatic" sciences.

The wide gap between the unconscious and the overconscious thinker is concealed by the growing sophistication of statistical and research techniques. Most of the literature introduced by the title "Methods" (in the social, behavioral or political sciences) actually deals with survey techniques and social statistics, and has little if anything to share with the crucial concern of "methodology," which is a concern with the logical structure and procedure of scientific enquiry. In a very crucial sense there is no methodology without *logos*, without thinking about thinking. And if a firm distinction is drawn—as it should be—between methodology and technique, the latter is no substitute for the former. One may be a wonderful researcher and manipulator of data, and yet remain an unconscious thinker. The view presented in this article is, then, that the profession as a whole is grievously impaired by methodological unawareness. The more we advance technically, the more we leave a vast, uncharted territory behind our backs. And my underlying complaint is that political scientists eminently lack (with exceptions) a training in logic—indeed in elementary logic.

I stress "elementary" because I do not wish to encourage in the least the overconscious thinker, the man who refuses to discuss heat unless he is given a thermometer. My sympathy goes, instead, to the "conscious thinker," the man who realizes the limitations of not having a thermometer and still manages to say a great deal simply by saying hot and cold, warmer and cooler. Indeed I call upon the conscious thinker to steer a middle course between crude logical mishandling on the one hand, and logical perfectionism (and paralysis) on the other hand. Whether we realize it or not, we are still swimming in a sea of naivete. And the study of comparative politics is particularly vulnerable to, and illustrative of, this unfelicitous state of affairs.

I. THE TRAVELLING PROBLEM

Traditional, or the more traditional, type of political science inherited a vast array of concepts which had been previously defined and refined—for better and for worse—by generations of philosophers and political theorists. To some extent, therefore, the traditional political scientist could afford to be an "unconscious thinker"—the thinking had already been done for him. This is even more the case with the country-by-country legalistic institutional approach, which does not particularly require hard thinking.[2] However, the new political science engages in reconceptualization. And this is even more the case, necessarily, with the new comparative expansion of the discipline.[3] There are many reasons for this *renovatio ab imis*.

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* An earlier draft, "Theory and Method in Comparative Politics," was submitted as a working paper to the IPSA Torino Round Table of September, 1969. I wish to thank, in this connection, the Agnelli Foundation which provided the grant for the Torino panel. I am particularly indebted to David Apter, Harry Eckstein, Carl J. Friedrich, Joseph LaPalombara, Felix Oppenheim and Fred W. Riggs for their critical comments. I am also very much obliged to the Concilium on International and Area Studies at Yale University, of which I was a fellow in 1966–67. This article is part of the work done under the auspices of the Concilium.

[1] C. Wright Mills, "On Intellectual Craftsmanship," in Llewellyn Gross (ed.), *Symposium on Sociological Theory* (New York: Harper & Row, 1959) p. 27 (My emphasis).

One is the very "expansion on politics." To some extent politics results *objectively* bigger on account of the fact that the world is becoming more and more politicized (more participation, more mobilization, and in any case more state intervention in formerly non-governmental spheres). In no small measure, however, politics is *subjectively* bigger in that we have shifted the focus of attention both toward the periphery of politics (vis-à-vis the governmental process), and toward its input side. By now—as Macridis puts it—we study everything that is "potentially political."[4] While this latter aspect of the expansion of politics is disturbing—it ultimately leads to the disappearance of politics—it is not a peculiar concern for comparative politics, in the sense that other segments of political science are equally and even more deeply affected.[5]

Aside from the expansion of politics, a more specific source of conceptual and methodological challenge for comparative politics is what Braibanti calls the "lengthening spectrum of political systems."[6] We are now engaged in world-wide, cross-area comparisons. And while there is an end to geographical size, there is apparently no end to the proliferation of political units. There were about 80 States in 1946; it is no wild guess that we may shortly arrive at 150. Still more important, the lengthening spectrum of political systems includes a variety of primitive, diffuse polities at very different stages of differentiation and consolidation.

Now, the wider the world under investigation, the more we need conceptual tools that are able to travel. It is equally clear that the pre-1950 vocabulary of politics was not devised for world-wide, cross-area travelling. On the other hand, and in spite of bold attempts at drastic terminological innovation,[7] it is hard to see how Western scholars could radically depart from the political experience of the West, i.e., from the vocabulary of politics which has been developed over millennia on the basis of such experience. Therefore, the first question is: how far, and how, can we travel with the help of the available vocabulary of politics?

By and large, so far we have followed (more or less unwitingly) the line of least resistance: broaden the meaning—and thereby the range of application—of the conceptualizations at hand. That is to say, the larger the world, the more we have resorted to *conceptual stretching*, or conceptual straining, i.e., to vague, amorphous conceptualizations. To be sure, there is more to it. One may add, for instance, that conceptual stretching also represents a deliberate attempt to make our conceptualizations value free. Another concurrent explication is that conceptual straining is largely a "boomerang effect" of the developing areas, i.e., a feedback on the Western categories of the diffuse polities of the Third

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[2] This is by no means a criticism of a comparative item by item analysis, and even less of the "institutional-functional" approach. On the latter see the judicious remarks of Ralph Braibanti, "Comparative Political Analytics Reconsidered," *The Journal of Politics*, 30 (February 1968), 44–49.

[3] For the various phases of the comparative approach see Eckstein's perceptive "Introduction," in H. Eckstein and D. E. Apter (eds.), *Comparative Politics* (Glencoe: Free Press, 1963).

[4] "Comparative Politics and the Study of Government: The Search for Focus," *Comparative Politics*, (October 1968), p. 81.

[5] On the "fallacy of inputism" see again the remarks of Roy C. Macridis, *loc. cit.*, pp. 84–87. In his words, "The state of the discipline can be summed up in one phrase: the gradual disappearance of the political." (p. 86). A cogent statement of the issue is Glenn D. Paige, "The Rediscovery of Politics," in J. D. Montgomery and W. I. Siffin (eds.), *Approaches to Development* (New York: McGraw Hill, 1966), p. 49 ff. My essay "From the Sociology of Politics to Political Sociology," in S. M. Lipset (ed.), *Politics and the Social Sciences* (New York: Oxford University Press, 1969), pp. 65–100, is also largely concerned with the fallacy of inputism viewed as a sociological reduction of politics.

[6] "Comparative Political Analytics Reconsidered," *loc. cit.*, pp. 36–37.

[7] The works of Fred W. Riggs are perhaps the best instance of such bold attempts. For a recent presentation see "The Comparison of Whole Political Systems," in R. T. Holt and J. E. Turner (eds.), *The Methodology of Comparative Research* (New York: Free Press, 1970), esp. pp. 95–115. While Riggs' innovative strategy has undeniable practical drawbacks, the criticism of Martin Landau ("A General Commentary," in Ralph Braibanti (ed.), *Political and Administrative Development* (Durham: Duke University Press, 1969), pp. 325–334.) appears somewhat unfair.

World.[8] These considerations notwithstanding, conceptual stretching does represent, in comparative politics, the line of least resistance. And the net result of conceptual straining is that our gains in extensional coverage tend to be matched by losses in connotative precision. It appears that we can cover more—in travelling terms—only by saying less, and by saying less in a far less precise manner.

A major drawback of the comparative expansion of the discipline is, then, that it has been conducive to indefiniteness, to undelimited and largely undefined conceptualizations. We do need, ultimately, "universal" categories—concepts which are applicable to any time and place. But nothing is gained if our universals turn out to be "no difference" categories leading to pseudo-equivalences. And even though we need universals, they must be *empirical* universals, that is, categories which somehow are amenable, in spite of their all-embracing very abstract nature, to empirical testing. Instead we seem to verge on the edge of *philosophical* universals, understood—as Croce defines them—as concepts which are by definition supra-empirical.[9]

That the comparative expansion of the discipline would encounter the aforementioned stumbling block was only to be expected. It was easy to infer, that is, that conceptual stretching would produce indefiniteness and elusiveness, and that the more we climb toward high-flown universals, the more tenuous the link with the empirical evidence. It is pertinent to wonder, therefore, why the problem has seldom been squarely confronted.

Taking a step back, let us begin by asking whether it is really necessary to embark in hazardous world-wide comparisons. This question hinges, in turn, on the prior question, Why compare? The unconscious thinker does not ask himself why he is comparing; and this neglect goes to explain why so much comparative work provides extensions of knowledge, but hardly a strategy for acquiring and validating new knowledge. It is not intuitively evident that to compare is to control, and that the novelty, distinctiveness and importance of comparative politics consists of a systematic testing, against as many cases as possible, of sets of hypotheses, generalizations and laws of the "if . . . then" type.[10] But if comparative politics is conceived as a method of control, then its generalizations have to be checked against "all cases," and therefore the enterprise must be—in principle—a global enterprise. So the reason for world-wide comparisons is not simply that we live in a wider world; it is also a methodological reason.

If two or more items are identical, we do not have a problem of comparability. On the other hand, if two or more items have nothing, or not enough in common, we rightly say that stones and rabbits cannot be compared. By and large, then, we obtain comparability when two or more items appear "similar enough," that is, neither identical nor utterly different. But this assessment offers little positive guidance. The problem is often outflanked by saying that we make things comparable. In this perspective to compare is "to assimilate," i.e., to discover deeper or fundamental similarities below the surface of secondary diversities. But this argument equally affords little mileage and conveys, moreover, the misleading suggestion that the trick resides in making the unlike look alike. Surely, then, we have here a major problem which cannot be disposed of with the argument that political theorists have performed decently with comparing since the time of Aristotle, and therefore that we should not get bogged by the question "What is comparable?" any more than our predecessors. This argument will not do on account of three differences.

In the first place if our predecessors were culture bound this implied that they travelled only as far as their personal knowledge allowed them to travel. In the second place, our predecessors hardly disposed of quantitive data and were not quantitatively oriented. Under both of these limitations they enjoyed the distinct advantage of having a substantive understanding of the things they were comparing. This is hardly possible on a world wide scale, and surely becomes impossible with the computer revolution. A few years ago Karl Deutsch predicted that by 1975 the informational requirements of political science would be satisfied by some "fifty million card-equivalents [of IBM standard cards] . . . and a total annual growth rate of perhaps as much as five million."[11] I find the estimate frightening, for computer technology and facilities are bound to flood us with masses of data for which no human mind can have any substantive grasp. But even if one shares the enthusiasm of Deutsch, it cannot be denied that we have here a gigantic, unprecedented problem.

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[8] On the boomerang effect of the developing areas more in the final section.

[9] More precisely in B. Croce, *Logica come Scienza del Concetto Puro,* (Bari: Laterza, 1942), pp. 13–17, universals are defined *ultrarappresentativi,* as being above and beyond any conceivable empirical representability.

[10] For the comparative method as a "method of control" see especially Arend Lijphart, *Comparative Politics and the Comparative Method,* (mimeographed) paper presented at the Torino IPSA Round Table, September, 1969. According to Lijphart the comparative method is a "method of discovering empirical relationships among variables" (p. 2); and I fully concur, except that this definition can be entered only at a later stage of the argument.

In the third place, our predecessors were far from being as unguided as we are. They did not leave the decision about what was homogenous—i.e., comparable—and what was heterogenous—i.e., non-comparable—to each man's genial insights. As indicated by the terminology, their comparisons applied to things belonging to "the same genus." That is to say, the background of comparability was established by the *per genus et differentiam* mode of analysis, i.e., by a taxonomical treatment. In this context, comparable means something which belongs to the same genus, species, or sub-species—in short to the same class. Hence the class provides the "similarity element" of comparability, while the "differences" enter as the species of a genus, or the sub-species of a species—and so forth, depending on how fine the analysis needs to be. However, and here is the rub, the taxonomical requisites of comparability are currently neglected, if not disowned.

We are now better equipped for a discussion of our initial query, namely, why the travelling problem of comparative politics has been met with the poor remedy of "conceptual stretching" instead of being squarely confronted. While there are many reasons for our neglect to attack the problem frontally, a major reason is that we have been swayed by the suggestion that our difficulties can be overcome by switching from "what is" questions to "how much" questions. The argument runs, roughly, as follows. As long as concepts point to differences of *kind,* i.e., as long as we pursue the either-or mode of analysis, we are in trouble; but if concepts are understood as a matter of more-or-less, i.e., as pointing to differences in *degree,* then our difficulties can be solved by measurement, and the real problem is precisely how to measure. Meanwhile—waiting for the measures—class concepts and taxonomies should be looked upon with suspicion (if not rejected), since they represent "an old fashioned logic of properties and attributes not well adapted to study quantities and relations."[12]

According to my previous analysis, a taxonomic unfolding represents a requisite condition for comparability, and indeed a background which becomes all the more important the less we can rely on a substantive familiarity with what is being compared. According to the foregoing argument, instead, quantification has no ills of its own; rather, it provides a remedy for the ills and inadequacies of the *per genus et differentiam* mode of analysis. My own view is that when we dismiss the so-called "old fashioned logic" we are plain wrong, and indeed the victims of poor logic—a view that I must now attempt to warrant.

II. QUANTIFICATION AND CLASSIFICATION

What is very confusing in this matter is the abuse of a quantitative idiom which is nothing but an idiom. All too often, that is, we speak of degrees and of measurement "not only without any actual measurements having been performed, but without any being projected, and even without any apparent awareness of what must be done before such measurements can be carried out."[13] For instance, in most standard textbooks one finds that nominal scales are spoken of as "scales of measurement."[14] But a nominal scale is nothing else than a qualitative classification, and I fail to understand what it is that a nominal scale does, or can, measure. To be sure classes can be given numbers; but this is simply a coding device for identifying items and has nothing to do with quantification. Likewise the incessant use of "it is a matter of degree" phraseology and of the "continuum" image leave us with qualitative-impressionistic statements which do not advance us by a hair's breadth toward quantification. In a similar vein we speak more and more of "variables" which are not variables in any proper sense, for they are not attributes permitting gradations and implying measurability. No harm necessarily follows if it pleases us to use the word variable as a synonym for the word concept; but we are only deluding ourselves if we really believe that by *saying* variable we *have* a variable.

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[11] "Recent Trends in Research Methods," in J. C. Charlesworth (ed.), *A Design for Political Science: Scope, Objectives and Methods* (Philadelphia: American Academy of Political and Social Science, 1966), p. 156.

[12] Carl F. Hempel, quoted in Don Martindale, "Sociological Theory and the Ideal Type," in Gross, *Symposium on Sociological Theory,* p. 87. Martindale aptly comments that "Hempel's judgments are made from the standpoint of the natural sciences." But the vein is not dissimilar when the statistically trained scholar argues that "whereas it is admittedly technically possible to think always in terms of attributes and dichotomies, one wonders how practical that is": Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964, p. 32).

[13] Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), p. 213.

[14] Eg., L. Festinger and D. Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Dryden Press, 1953); and Selltiz, Jahoda *et al., Research Methods in Social Relations* (rev. ed., New York: Holt, Rinehart & Winston, 1959).

All in all, coquetting (if not cheating) with a quantitative idiom grossly exaggerates the extent to which political science is currently amenable to quantification, and, still worse, obfuscates the very notion of quantification. The dividing line between the jargon and the substance of quantification can be drawn very simply: quantification begins with numbers, and when numbers are used in relation to their arithmetical properties. To understand, however, the multifaceted complexities of the notion beyond this dividing line is a far less simple matter. Nevertheless one may usefully distinguish—in spite of the close interconnections—among three broad areas of meaning and application, that is, between quantification as i) measurement, ii) statistical manipulation and, iii) formal mathematical treatment.

In political science we generally refer to the first meaning. That is to say, far more often than not the quantification of political science consists of (a) attaching numerical values to items (pure and simple measurement), (b) using numbers to indicate the rank order of items (ordinal scales) and (c) measuring differences or distances among items (interval scales).[15]

Beyond the stage of measurement we do own, in addition, powerful statistical techniques not only for protecting ourselves against sampling and measurement errors, but also for establishing significant relationships among variables. However, statistical processing enters the scene only when sufficient numbers have been pinned on sufficient items, and becomes central to the discipline only when we dispose of variables which measure things that are worth measuring. Both conditions—and especially the latter—are hard to meet.[16] Indeed, a cross-examination of our statistical findings in terms of their theoretical significance—and/or of a "more relevant" political science—shows an impressive disproportion between bravura and relevance. Unfortunately, what makes a statistical treatment theoretically significant has nothing to do with statistics.

As for the ultimate stage of quantification—formal mathematical treatment—it is a fact that, so far, political science and mathematics have engaged only "in a sporadic conversation."[17] It is equally a fact that we seldom, if ever, obtain isomorphic correspondences between empirical relations among things and formal relations among numbers.[18] We may well disagree about future prospects,[19] or as to whether it makes sense to construct formalized systems of quantitatively well defined relationships (mathematical models) so long as we wander in a mist of qualitatively ill-defined concepts. If we are to learn, however, from the mathematical development of economics, the evidence is that it "always lagged behind its qualitative and conceptual improvement."[20] And my point is, precisely, that this is not a casual sequence. It is for a very good reason that the progress of quantification should lag—in whatever discipline—behind its qualitative and conceptual progress.

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[15] There is some question as to whether it can really be held that ordinal scales are scales of measurement: most of our rank ordering occurs without having recourse to numerical values, and whenever we do assign numbers to our ordered categories, these numbers are arbitrary. However, there are good reasons for drawing the threshold of quantification between nominal and ordinal scales rather than between ordinal and interval scales. (See Edward R. Tufte, "Improving Data Analysis in Political Science," *World Politics*, 21 (July 1969), esp. p. 645.) On the other hand, even if the gap between ordinal scales and interval measurement is not as wide in practice as it is in theory, nonetheless from a mathematical point of view the interesting scales are the interval and even more, of course, the cardinal scales.

[16] Otherwise the comparative method would largely consist of the statistical method, for the latter surely is a stronger technique of control than the former. The difference and the connections are cogently discussed by Lijphart, "Comparative Politics and and the Comparative Method," *op. cit.*

[17] Oliver Benson, "The Mathematical Approach to Political Science," in J. C. Charlesworth (ed.), *Contemporary Political Analysis* (New York: Free Press, 1967), p. 132. The chapter usefully reviews the literature. For an introductory treatment see Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965). An illuminating discussion on how quantification enters the various social sciences is in Daniel Lerner (ed.), *Quantity and Quality* (Glencoe: Free Press, 1961), *passim.*

[18] A classic example is the (partial) mathematical translation of the theoretical system of *The Human Group* of George C. Homans by Herbert A. Simon, *Models of Man* (New York: Wiley, 1967), Chap. 7. No similar achievement exists in the political science field. To cite three significant instances, political science issues are eminently lacking in Kenneth J. Arrow, "Mathematical Models in the Social Sciences," in D. Lerner and H. D. Lasswell (eds.), *The Policy Sciences* (Stanford: Stanford University Press, 1951), Chap. 8; in the contributions collected in P. F. Lazarsfeld (ed.), *Mathematical Thinking in the Social Sciences* (Glencoe: Free Press, 1954); in J. G. Kemeny and J. L. Snell, *Mathematical Models in the Social Sciences* (Boston: Ginn, 1962).

[19] Perhaps the mathematical leap of the discipline is just around the corner waiting for nonquantitative developments. If one is to judge, however, from the "mathematics of man" issue of the *International Social Science Bulletin* introduced by Claude Levi-Strauss (IV, 1954), this literature is very deceiving. More interesting is John G. Kemeny, "Mathematics without Numbers," in Lerner, *Quantity and Quality*, pp. 35–51; and the modal logic developed by the Bourbaki group,

In this messy controversy about quantification and its bearing on standard logical rules we simply tend to forget that *concept formation stands prior to quantification*. The process of thinking inevitably begins with a qualitative (natural) language, no matter at which shore we shall subsequently land. Correlatively, there is no ultimate way of bypassing the fact that human understanding—the way in which our mind works—requires cut-off points which basically correspond (in spite of all subsequent refinements) to the slices into which a natural or qualitative language happens to be divided.

There is a fantastic lack of perspective in the argument that these cut-off points can be obtained via statistical processing, i.e., by letting the data themselves tell us where to draw them. For this argument applies only *within* the frame of conceptual mappings which have to tell us first of what reality is composed. Let it be stressed, therefore, that long before having data which can speak for themselves the fundamental articulation of language and of thinking is obtained logically—by cumulative conceptual refinement and chains of coordinated definitions—not by measurement. Measurement of what? We cannot measure unless we know first what it is that we are measuring. Nor can the degrees of something tell us what a thing is. As Lazarsfeld and Barton neatly phrase it, "before we can investigate the presence or absence of some attribute . . . or before we can rank objects or measure them in terms of some variable, *we must form the concept of that variable*."[21]

The major premise is, then, that quantification enters the scene after, and only after, having formed the concept. The minor premise is that the "stuff" of quantification—the things underpinned by the numbers—cannot be provided by quantification itself. Hence the rules of concept formation are independent of, and cannot be derived from, the rules which govern the treatment of quantities and quantitative relations. Let us elaborate on this conclusion.

In the first place, if we never really have "how much" findings—in the sense that the prior question always is how much *in what*, in what conceptual container—it follows from this that how much quantitative findings are an internal element of "what is" qualitative questions: the claim that the latter should give way to the former cannot be sustained. It equally follows, in the second place, that "categoric concepts" of the either-or type cannot give way to "gradation concepts" of the more-than-less-than type.

What is usually lost sight of is that the either-or type of logic is the very logic of classification building. Classes are required to be mutually exclusive, i.e., class concepts represent characteristics which the object under consideration must either have or lack. Two items being compared must belong first to the same class, and either have or not have an attribute; and only if they have it, the two items can be matched in terms of which has it *more* or *less*. Hence the logic of gradation belongs to the logic of classification. More precisely put, the switch from classification to gradation basically consists of replacing the signs "same-different" with the signs "same-greater-lesser," i.e., consists of introducing a quantitative differentiation within a qualitative sameness (of attributes). Clearly, then, the sign "same" established by the logic of classification is the requisite condition of introducing the signs "plus-minus."

The retort tends to be that this is true only as long as we persist in thinking in terms of attributes and dichotomies. But this rejoinder misses the point that—aside from classifying—we dispose of no other unfolding technique. Indeed, the taxonomical exercise "unpacks" concepts, and plays a non-replaceable role in the process of thinking in that it decomposes mental compounds into orderly and manageable sets of component units. Let it be added that at no stage of the methodological argument does the taxonomical unpacking lose weight and impor-

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Eléments de Mathématique, appearing periodically (Paris: Hermann). For a general treatment see J. G. Kemeny, J. L. Snell, G. L. Thompson, *Introduction to Finite Mathematics* (Englewood Cliffs: Prentice Hall, 1957).

[20] Joseph J. Spengler, "Quantification in Economics: Its History," in Lerner, *Quantity and Quality,* p. 176. Spengler equally points out that "the introduction of quantitative methods in economics did not result in striking discoveries" (*ibid.*). While formal economic theory is by now highly isomorphic with algebra, mathematical economics has added little to the predictive power of the discipline and one often has the impression that we are employing guns to kill mosquitos.

[21] "Qualitative Measurement in the Social Sciences: Classifications, Typologies and Indices," in D. Lerner and H. D. Lasswell (eds.), *The Policy Sciences, op. cit.,* p. 155 (my emphasis).

tance. As a matter of fact, the more we enter the stage of quantification, the more we need unidimensional scales and continua; and dichotomous categorizations serve precisely the purpose of establishing the ends, and thereby the uni-dimensionality, of each continuum.

Having disposed of the fuzziness brought about by the abuse of a quantitative idiom, attention should immediately be called to the fact-finding side of the coin. For my emphasis on concept formation should not be misunderstood to imply that my concern is more theoretical than empirical. This is not so, because the concepts of any social science are not only the elements of a theoretical system; they are equally, and just as much, data containers. Indeed data is information which is distributed in, and processed by, "conceptual containers." And since the non-experimental sciences basically depend on fact-finding, i.e., on reports about external (not laboratory) observables, the empirical question becomes what turns a concept into a valuable, indeed a valid, fact finding container.

The reply need not be far-fetched: the lower the discriminating power of a conceptual container, the more the facts are misgathered, i.e., the greater the misinformation. Conversely, the higher the discriminating power of a category, the better the information. Admittedly, in and by itself this reply is not very illuminating, for it only conveys the suggestion that for fact-finding purposes it is more profitable to exaggerate in over-differentiation than in over-assimilation. The point is, however, that what establishes, or helps establish, the discriminating power of a category is the taxonomical infolding. Since the logical requirement of a classification is that its classes should be mutually exclusive and jointly exhaustive, it follows from this that the taxonomical exercise supplies an orderly series of well sharpened categories, and thereby the basis for collecting adequately precise information. And this is indeed how we know whether, and to what extent, a concept has a fact-gathering validity.

Once again, then, it appears that we have started to run before having learned how to walk. Numbers must be attached—for our purposes—to "things," to facts. How are these things, or facts, identified and collected? Our ultimate ambition may well be to pass from a science "of species" to a science of "functional co-relations."[22] The question is whether we are not repudiating a science of species in exchange for nothing. And it seems to me that premature haste combined with the abuse of a quantitative idiom is largely responsible not only for the fact that much of our theorizing is muddled, but also for the fact that much of our research is trivial and wasteful.

Graduate students are being sent all over the world—as LaPalombara vividly puts it—on "indiscriminate fishing expeditions for data."[23] These fishing expeditions are "indiscriminate" in that they lack taxonomical backing; which is the same as saying that they are fishing expeditions without adequate nets. The researcher sets out with a "checklist" which is, at best, an imperfect net of his own. This may be an expedient way of handling his private research problems, but remains a very inconvenient strategy from the angle of the additivity and the comparability of his findings. As a result, the joint enterprise of comparative politics is menaced by a growing potpourri of disparate, non-cumulative and—in the aggregate—misleading morass of information.

All in all, and regardless of whether we rely on quantitative data or on more qualitative information, in any case the problem is the same, namely, to construct fact-finding categories that own sufficient discriminating power.[24] If our data containers are blurred, we never know to what extent and on what grounds the "unlike" is made "alike." If so, quantitative analysis may well provide more misinformation than qualitative analysis, especially on account of the aggravating circumstance that quantitative misinformation can be used without any substantive knowledge of the phenomena under consideration.

To recapitulate and conclude, I have argued that the logic of either-or cannot be replaced by the logic of more-and-less. Actually the two logics are complementary, and each has a legitimate field of application. Correlatively, polar oppositions and dichotomous confrontations cannot be dismissed: they are a necessary step in the process of concept formation. Equally, impatience with classification is totally unjustified. Rather, we often confuse a mere enumeration (or checklist) with a classification, and many so called classifications fail to meet the minimal requirements for what they claim to be.

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[22] Harold D. Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), pp. XVI–XVII.

[23] "Macrotheories and Microapplications in Comparative Politics," *Comparative Politics,* (October 1968), p. 66.

[24] It hardly needs to be emphasized that census data—and for that matter most of the data provided by external agencies—are gathered by conceptual containers which hopelessly lack discrimination. The question with our standard variables on literacy, urbanization, occupation, industrialization, and the like, is whether they really measure common underlying phenomena. It is pretty obvious that, across the world, they do not; and this quite aside from the reliability of the data gathering agencies.

The overconscious thinker takes the view that if the study of politics has to be a "science," then it has to be Newton (or from Newton all the way up to Hempel). But the experimental method is hardly within the reach of political science (beyond the format of small group experimentation) and the very extent to which we are systematically turning to the comparative method of verification points to the extent to which no stronger method—including the statistical method—is available. If so, our distinctive and major problems begin where the lesson of the more exact sciences leaves off. This is tantamount to saying that a wholesale acceptance of the logic and methodology of physics may well be self-defeating, and is surely of little use for our distinctive needs. In particular, and whatever their limits, classifications remain the requisite, if preliminary, condition for any scientific discourse. As Hempel himself concedes, classificatory concepts do lend themselves to the description of observational findings and to the formulation of initial, if crude, empirical generalizations.[25] Moreover, a classificatory activity remains the basic instrument for introducing analytical clarity in whatever we are discussing, and leads us to discuss one thing at a time and different things at different times. Finally, and especially, we need taxonomical networks for solving our fact-finding and fact-storing problems. No comparative science of politics is plausible—on a global scale—unless we can draw on extensive *information* which is sufficiently *precise* to be meaningfully *compared*. The requisite condition for this is an adequate, relatively stable and, thereby, *additive filing system*. Such a filing system no longer is a wild dream, thanks to computer technology and facilities—except for the paradoxical fact that the more we enter the computer age, the less our fact-finding and fact-storing methods abide by any logically standardized criterion. Therefore, my concern with taxonomies is also a concern with 1) the data side of the question, and 2) our failure to provide a filing system for computer exploitation. We *have* entered the computer age—but with feet of clay.

III. THE LADDER OF ABSTRACTION

If quantification cannot solve our problems, in that we cannot measure before conceptualizing, and if, on the other hand, "conceptual stretching" is dangerously conducive to the Hegelian night in which all the cows look black (and eventually the milkman is taken for a cow), then the issue must be joined from its very beginning, that is, on the grounds of concept formation.

A few preliminary cautions should be entered. Things conceived or meaningfully perceived, i.e., concepts, are the central elements of propositions, and—depending on how they are named—provide in and by themselves guidelines of interpretation and observation. It should be understood, therefore, that I shall implicitly refer to the conceptual element problems which in a more extended treatment actually and properly belong to the rubric "propositions." By saying concept formation I implicitly point to a proposition-forming and problem-solving activity. It should also be understood, in the second place, that my focus will be on those concepts which are crucial to the discipline, that is, the concepts which Bendix describes as "generalizations in disguise."[26] In the third place, I propose to concentrate on the vertical members of a conceptual structure, that is, on 1) *observational terms*, and 2) the vertical disposition of such terms along a *ladder of abstraction*.

While the notion of abstraction ladder is related to the problem of the levels of analysis, the two things do not coincide. A highly abstract level of analysis may not result from "ladder climbing. Indeed a number of universal conceptualizations are not abstracted from observables: they are "theoretical terms" defined by their systemic meaning.[27] For instance the meaning of isomorphism, homeostasis, feedback, entrophy, etc., is basically defined by the part that each concept plays in the whole theory. In other instances, however, we deal with "observational terms," that is, we arrive at highly abstract levels of conceptualization via ladder climbing, via abstractive inferences from observables. For instance, terms such as group, communication, conflict, and decision can either be used in a very abstract or in a very concrete meaning, either in some very distant relation to observables or with reference to direct observations. In this case we have, then, "empirical concepts" which can be located at, and moved along, very different points of a ladder of abstraction. If so, we have the problem of assessing the level of abstraction at which observational or (in this sense) empirical concepts are located, and the rules of transformation thus resulting. And this seems to be the pertinent focus for the issue under consideration, for our fundamental problem is how to make extensional gains (by climbing the abstraction ladder) without having to suffer unnecessary losses in precision and empirical testability.

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[25] *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952), p. 54.

[26] Reinhard Bendix, "Concepts and Generalizations in Comparative Sociological Studies," *American Sociological Review*, 28 (1963), p. 533.

[27] See Abraham Kaplan, *The Conduct of Inquiry*, pp. 56-57, 63-65. According to Hempel theoretical terms "usually purport to not directly observable entities and their characteristics. . . . They function . . . in scientific theories intended to explain generalizations": "The Theoretician's Dilemma," in Feigl, Scriven and Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* (Minneapolis: University of Minnesota Press, 1958), vol. II, p. 42. While it is admittedly difficult to draw a neat division between theoretical and observational terms, it is widely recognized that the former cannot be reduced to, nor derived from, the latter. For a recent assessment of the controversy, see A. Meotti, "L'Eliminazione dei Termini Teorici," in *Rivista di Filosofia*, 2 (1969), pp. 119-134.

The problem can be neatly underpinned with reference to the distinction, and relation, between the *extension* (denotation) and *intension* (connotation) of a term. A standard definition is as follows: "The extension of a word is the class of *things* to which the word applies; the intension of a word is the collection of *properties* which determine the things to which the word applies."[28] Likewise, the denotation of a word is the totality of objects indicated by that word; and the connotation is the totality of characteristics anything must possess to be in the denotation of that word.[29]

Now, there are apparently two ways of climbing a ladder of abstraction. One is to broaden the extension of a concept by diminishing its attributes or properties, i.e., by reducing its connotation. In this case a more "general," or more inclusive, concept can be obtained without any loss of precision. The larger the class, the lesser its differentiae; but those differentiae that remain, remain precise. Moreover, following this procedure we obtain conceptualizations which, *no matter how all-embracing, still bear a traceable relation to a collection of specifics*, and—out of being amenable to identifiable sets of specifics—lend themselves to empirical testing.

On the other hand, this is hardly the procedure implied by "conceptual stretching," which adds up to being an attempt to augment the extension without diminishing the intension: *the denotation is extended by obfuscating the connotation.* As a result we do not obtain a more general concept, but its counterfeit, a mere generality (where the pejorative "mere" is meant to restore the distinction between correct and incorrect ways of subsuming a term under a broader genus.) While a general concept can be said to represent a collection of specifics, a mere generality cannot be underpinned, out of its indefiniteness, by specifics. And while a general concept is conducive to scientific "generalizations," mere generalities are conducive only to vagueness and conceptual obscurity.

The rules for climbing and descending along a ladder of abstraction are thus very simple rules—in principle. We make a concept more abstract and more general by lessening its properties or attributes. Conversely, a concept is specified by the addition (or unfolding) of qualifications, i.e., by augmenting its attributes or properties. If so, let us pass on to consider a ladder of abstraction as such. It is self-evident that along the abstraction ladder one obtains very different degrees of inclusiveness and, conversely, specificity. These differences can be usefully underpinned—for the purposes of comparative politics—by distinguishing three levels of abstraction, labeled, in shorthand, HL (high level), ML (medium level), and LL (low level).

High level categorizations obtain universal conceptualizations: whatever connotation is sacrificed to the requirement of global denotation—either in space, time, or even both.[30] HL concepts can also be visualized as the ultimate genus which cancels all its species. Descending a step, medium level categorizations fall short of universality and thus can be said to obtain general classes: at this level not all differentiae are sacrificed to extensional requirements. Nonetheless, ML concepts are intended to stress similarities at the expense of uniqueness, for at this level of abstraction we are typically dealing with generalizations. Finally, low level categories obtain specific, indeed configurative conceptualizations: here denotation is sacrificed to accuracy of connotation. One may equally say that with LL categories the differentiae of individual settings are stressed above their similarities: so much so that at this level definitions are often contextual.

A couple of examples may be usefully entered. In a perceptive essay which runs parallel to my line of thinking Neil J. Smelser makes the point that, for purposes of comparability, "staff is more satisfactory than administration . . . , and administration is more satifactory than civil service."[31] This is so, according to Smelser, because the concept of civil service "is literally useless in connection with societies without a formal state or governmental apparatus." In this respect "the concept of administration is somewhat superior . . . but even this term is quite culture-bound." Hence the more helpful term is "Weber's concept of staff . . . since it can encompass without embarassment various political arrangements . . ."[32] In my own terms the argument would be rephrased as follows. In the field of so-called comparative public administration, "staff" is the high level universal category. "Administration" is still a good travelling category, but falls short of universal applicability in that it retains some of the attributes associated with the more specific notion of "bureaucracy." Descending the ladder of abstraction further we then find "civil service," which is qualified by its associations with the modern State. Finally, and to pursue the argument all the way down to the low level of abstraction, a comparative study of, say, French and English state employees will discover their unique and distinguishing traits and would thus provide contextual definitions.

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[28] I quote from Wesley C. Salmon, *Logic* (Englewood Cliffs: Prentice-Hall, 1963), pp. 90–91. The distinction is more or less the same in any textbook of logic.

[29] "Connotation" is also applied, more broadly, to the associations, or associated conceptions brought to mind by the use of a word. As indicated by the text, I intend here the narrower meaning.

[30] The space and time dimensions of concepts are often associated with the geography versus history debate. I would rather see it as the "when goes with when?" question, that is, as a calendar time versus historical time dilemma. But this line of development cannot be pursued here.

The example suggested by Smelser is fortunate in that we are offered a choice of terms, so that (whatever the choice) a different level of abstraction can be identified by a different denomination. The next example is illustrative, instead, of the far less fortunate situation in which we may have to perform across the whole ladder of abstraction with one and same term. In illustrating his caution that many concepts are "generalizations in disguise," Bendix comes across such a simple concept as "village." Yet he notes that the term village may be misleading when applied to Indian society, where "the minimum degree of cohesion commonly associated with this term is absent."[33] Even in such a simple case, then, a scholar is required to place the various associations of "village" along an abstraction ladder in accord with the travelling extension afforded by each connotation.

Clearly, there is no hard and fast dividing line between levels of abstraction. Borders can only be drawn very loosely; and the number of slices into which the ladder is divided largely depends on how fine one's analysis needs to be. Three slices are sufficient, however, for the purposes of logical analysis. And my major concern is, in this connection, with what goes on at the upper end of the ladder, at the crucial juncture at which we cross the border between medium level general concepts and high level universals. The issue may be formulated as follows: how far up can an observational term be pushed without self-denying results?

In principle the extension of a concept should not be broadened beyond the point at which at least one relatively precise connotation (property or attribute) is retained. In practice, however, the requirement of positive identification may be too exacting. But even if no minimal positive identification can be afforded, I do not see how we can renounce the requirement of negative identification. The crucial distinction would thus be between 1) concepts defined by negation or *ex adverso,* i.e., by saying what they are *not,* and 2) concepts *without negation,* i.e., no-opposite concepts, conceptions without specified termination or boundaries. The logical principle involved in this distinction is *omnis determinatio est negatio,* that is, any determination involves a negation. According to this principle the former concepts are, no matter how broad, *determinate;* whereas the latter are indeterminate, literally *without termination.*

If this principle is applied to the climbing process along a ladder of abstraction, and precisely to the point at which ML categories are turned into HL universals, in the first instance we obtain *empirical universals,* whereas in the second instance we obtain universals which lack empirical value—*pseudo-universals* for an empirical science. The reason for this is that a concept qualified by a negation may, or may not, be found to apply to the real world; whereas a non-bounded concept always applies by definition: having no specified termination, there is no way of ascertaining whether it applies to the real world or not. An empirical universal is such because it still points to *something;* whereas a non-empirical universal indiscriminately points to *everything* (as any researcher on the field soon discovers).

The group concept lends itself nicely as an illustration of the foregoing (other examples will be discussed in greater detail later), and is very much to the point in that it represents the first large scale attempt to meet the travelling problem of comparative politics. In the group theory of politics (Bentley, David Truman, and Earl Lathan being the obvious references) it is clear enough that "group" becomes an all-embracing category: not only an analytical construct (as the queer and unclear terminology of the discipline would have it); but definitely a universal construct. However, we are never really told what group *is not.* Not only "group" applies *everywhere,* as any universal should; it equally applies to *everything,* that is, never and nowhere shall we encounter non-groups.[34] If so, how is it that the group theory of politics has been followed—in the fifties—by a great deal of empirical research? The reply is that the research was not guided by the universal construct but by intuitive concrete conceptualizations. Hence the "indefinite group" of the theory, and the "concrete groups" of the research, fall wide apart. The unfortunate consequences are not only that the research lacks theoretical backing (for want of medium level categories, and especially of a taxonomic framework), but that the vagueness of the theory has no fit for the specificity of the findings. We are thus left with a body of literature that gives the frustrating feeling of dismantling theoretically whatever it discovers empirically.

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[31] "Notes on the Methodology of Comparative Analysis of Economic Activity," *Transactions of the Sixth World Congress of Sociology,* 1967, International Sociological Association, vol. II, p. 103.

[32] *Ibid.*

[33] Bendix, "Concepts and Generalizations. . . ," p. 536.

There is, then, a break-off point in the search for universal inclusiveness beyond which we have, theoretically, a "nullification of the problem" and, empirically, what may be called an "empirical vaporization." This is the point at which a concept is not even determined *ex adverso.* By saying that no-opposite universals are of no empirical use I do not imply that they are utterly useless. But I do wish to say that whenever notions such as groups or—as in my subsequent examples—pluralism, integration, participation, and mobilization, obtain no termination, i.e., remain indeterminate, they provide only tags, chapter headings, i.e., the main entries of a filing system. From an empirical point of view pseudo-universals are only funnels of approach and can only perform, so to speak, an allusive function.

Turning to the middle slice—the fat slice of the medium level categories—it will suffice to note that at this level we are required to perform the whole set of operations that some authors call "definition by analysis," that is, the process of defining a term by finding the genus to which the object designated by the word belongs, and then specifying the attributes which distinguish such object from all the other species of the same genus. When Apter complains that our "analytical categories are too general when they are theoretical, and too descriptive where they are not,"[35] I understand this complaint to apply to our disorderly leaps from observational findings all the way up to universal categories—and vice versa—by-passing as it were the stage of definition by analysis. Apter is quite right in pleading for "better intermediate analytical categories." But these intermediate categories cannot be constructed, I fear, as long as our contempt for the taxonomical exercise leaves us with an atrophied medium level of abstraction.

The low level of abstraction may appear uninteresting to the comparative scholar. He would be wrong, however, on two counts. First, when the comparative scholar is engaged in field work, the more his fact-finding categories are brought down to this level, the better his research. Second, it is the evidence obtained nation-by-nation, or region-by-region (or whatever the unit of analysis may be) that helps us decide which classification works, or which new criterion of classification should be developed.

While classifying must abide by logical rules, logic has nothing to do with the usefulness of a classificatory system. Botanists, mineralogists and zoologists have not created their taxonomical trees as a matter of mere logical unfolding; that is, they have not imposed their "classes" upon their animals, any more than their animals (flowers or minerals) have imposed themselves upon their classifiers. Let it be added that the information requirements of such an unsettled science as a science of politics can hardly be satisfied by single-purpose classifications (not to mention single-purpose checklists). As I have stressed, we desperately need standard fact-finding and fact-storing containers (concepts). But this standardization is only possible and fruitful on the basis of "multi-purpose" and, at the limit, all-purpose classifications. Now, whether a classification may serve multiple purposes, and which classification fits this requirement best, this is something we discover inductively, that is, starting from the bottom of the ladder of abstraction.

The overall discussion is recapitulated in Table 1 with respect to its bearing on the problems of comparative politics. A few additional comments are in order. In the first place, reference to three levels of abstraction brings out the inadequacy of merely distinguishing between

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[34] This criticism is perhaps unfair to David Truman's *The Governmental Process* (New York: Knopf, 1951). However, in spite of its penetrating anatomy the pace of the enquiry is set by the sentence that "an excessive preoccupation with definition will only prove a handicap" (p. 23). For a development of this line of criticism see G. Sartori, "Gruppi di Pressione o Gruppi di Interesse?," *Il Mulino,* 1959, pp. 7–42.

[35] David E. Apter, "Political Studies and the Search for a Framework," (pp. 15–16 mns.) to be published in C. Allen, W. Johnson (eds.), *African Perspectives,* Cambridge University Press.

TABLE 1. LADDER OF ABSTRACTION

Levels of Abstraction	Major Comparative Scope and Purpose	Logical and Empirical Properties of Concepts
HL: *High Level Categories* Universal conceptualizations	Cross-area comparisons among heterogeneous contexts (global theory)	Maximal extension Minimal intension Definition by negation
ML: *Medium Level Categories* General conceptualizations and taxonomies	Intra-area comparisons among relatively homogeneous contexts (middle range theory)	Balance of denotation with connotation Definition by analysis, i.e. per genus et differentiam
LL: *Low Level Categories* Configurative conceptualizations	Country by country analysis (narrow-gauge theory)	Maximal intension Minimal extension Contextual definition

"broad" and "narrow" meanings of a term.[36] For this does not clarify, whenever this is necessary, whether we distinguish, 1) between HL universal and ML general conceptualizations, or 2) between ML genuses and species or, 3) between ML and LL categories, or even 4) between HL universal and LL configurative conceptualizations.

In the second place, and more important, reference to the ladder of abstraction forcibly highlights the drastic loss of logical articulation, indeed the gigantic leap, implied by the argument that *all* differences are "a matter of degree." This cannot be conceded, to begin with, at the level of universal categories. But all differences cannot be considered a matter of more-or-less at the medium level either. At the top we inevitably begin with opposite pairs, with polar opposites, and this is tantamount to saying that the top ML categories definitely and only establish differences in *kind*. From here downwards definitions are obtained via the logic of classification, and this implies that a logic of gradation cannot be applied as long as we establish differences between species. Differences in degree obtain only after having established that two or more objects have the same attributes or properties, i.e., belong to the same species. Indeed, it is only *within* the same class that we are entitled —and indeed required—to ask which object has more or less of an attribute or property.

In principle, then, it is a fallacy to apply the logic of gradation whenever ladder climbing (or descending) is involved. If we are reminded that along the ladder we augment the extension by diminishing the denotation (and vice versa), what is at stake here is the presence or absence of a given property; and this is not a matter of degree, but a matter of establishing the level of abstraction. Hence it is only after having settled at a given level of abstraction that considerations of more-and-less correctly apply. And the rule of thumb seems to be that the higher the level of abstraction, the less a degree language applies (as anything but a metaphor); whereas the lower level of abstraction, the more a degree optics correctly and necessarily applies, and the more we profit from graduation concepts.

In the third place, and equally important, reference to the ladder of abstraction casts many doubts on the optimistic view—largely shared by the methodological literature—that "The more universal a proposition, i.e., the greater the number of events a proposition accounts for, the more potential falsifiers can be found, and the more informative is the proposition."[37] The sentence suggests a simultaneous and somewhat natural progression of universality, falsifiers and informative content. It seems to me, instead, that reference to the correct technique of ladder climbing (and descending) confronts us at all points with choosing between range of explanation (thereby including the explanation of the relationships among the items under investigation), and accuracy of description (or informative accuracy). By saying that the "informative content" of a proposition grows by climbing the abstraction ladder, we should not be misled into understanding that we are supplying more descriptive information. Hence it is dubious whether we are really supplying more potential falsifiers (let alone the danger of "overly universal" propositions of no informative value for which falsifiers cannot be found).

Before concluding it should not pass unnoticed that in this section I have never used the word "variable," nor mentioned operational definitions, nor invoked indicators. Equally, my reference to gradation concepts and to considerations of more-or-less has been, so far, entirely pre-quantitative. What is noteworthy, then, is the length that has been travelled before entering the problems which seem to monopolize our methodological awareness. There is nothing wrong, to be sure, in taking up an argument at whichever point we feel that we have something to say—except that the tail of the methodological argument should not be mistaken for its beginning. Since I have taken up the issue at an early stage, I cannot possibly carry it through to its end. It behooves me, nonetheless, to indicate how I would plug what I have said into what shall have to remain unsaid.[38]

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[36] The same caution applies to the distinctions between micro and macro, or between molecular and molar. These distinctions are insufficient for the purpose of underpinning the level of analysis.

[37] I quote Erik Allardt, "The Merger of American and European Traditions of Sociological Research: Contextual Analysis," *Social Science Information*, 1 (1968), p. 165. But the sentence is illustrative of a current mood.

For one thing, it should be understood that by considering concepts—the genus—I have not excluded the consideration of variables, which are a species. That is, a variable is still a concept; but a concept is not necessarily a variable. If all concepts could be turned into variables, the difference could be considered provisional. Unfortunately, as a scholar well versed in quantitative analysis puts it, "all the most interesting variables are nominal."[39] Which is the same as saying that all the most interesting concepts are not variables in the proper, strict sense of implying "the possibility of measurement in the most exact sense of the word."[40]

A closely linked and similar argument applies to the operationist requirement. Just as concepts are not necessarily variables, definitions are not necessarily operational. The definitional requirement for a concept is that its *meaning* is declared, while operational definitions are required to state the conditions, indeed the operations, by means of which a concept can be *verified* and, ultimately, measured. Accordingly we may usefully distinguish between definition of meaning and operational definition. And while it is obvious that an operational definition still is a declaration of meaning, the reverse is not true.

The contention often is that definition of meaning represents a pre-scientific age of definition, which should be superseded in scientific discourse by operational definitions. However, this contention can hardly meet the problems of concept formation, and indeed appears to ignore them. As the ladder of abstraction scheme helps to underline, among the many possible ways and procedures of defining the *ex adverso* definitions and taxonomic unfoldings (or definition by analysis) some correspond to different levels of analysis and play, at each level, a non-replaceable role. Moreover operational definitions generally entail a drastic curtailment of meaning for they can only maintain those meanings that comply with the operationist requirement. Now, we are surely required to reduce ambiguity by cutting down the range of meanings of concepts. But the operational criterion of reducing ambiguity entails drastic losses in conceptual richness and in explanatory power. Take, for instance, the suggestion that "social class' should be dismissed and replaced by a set of operational statements relating to income, occupation, educational level, etc. If the suggestion were adopted wholesale, the loss of conceptual substance would be not only considerable, but unjustified. The same applies, to cite another instance, to "power." To be concerned with the measurement of power does not imply that the meaning of the concept should be reduced to what can be measured about power—the latter view would make human behavior in whatever collective sphere almost inexplicable.

It should be understood, therefore, that operational definitions implement, but do not replace, definitions of meaning. Indeed there must be a conceptualization before we engage in operationalization. As Hempel recommends, operational definitions should not be "emphasized to the neglect of the requirement of systematic import."[41] This is also to say that definitions of meaning of theoretical import, hardly operational definitions, account for the dynamics of intellectual discovery and stimulation. Finally, it should be understood that empirical testing occurs before, and also without, operational definitions. Testing is any method of checking correspondence with reality by the use of pertinent observations; hence the decisive difference brought about by operationalization is verification, or falsification, by measurement.[42]

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[38] In this latter connection an excellent reader still is P. F. Lazarsfeld and M. Rosenberg (eds.), *The Language of Social Research* (Glencoe: The Free Press, 1955). See also its largely revised and updated revision, R. Boudon and P. F. Lazarsfeld, *Méthodes de la Sociologie,* 2 Vols. (Paris and La Haye: Mouton, 1965–1966).

[39] Richard Rose, "Social Measure and Public Policy in Britain—The Empiricizing Process," mns. p. 8.

[40] Lazarsfeld and Barton in Lerner and Lasswell, *The Policy Sciences,* p. 170. This notably excludes, for the authors, the application of "variable" to items that can be ranked but not measured.

[41] *Fundamentals of Concept Formation in Empirical Science,* p. 60. At p. 47 Hempel writes: "it is precisely the discovery of concepts with theoretical import which advances scientific understanding; and such discovery requires scientific inventiveness and cannot be replaced by the—certainly indispensable, but also definitely insufficient—operationist or empiricist requirement of empirical import alone."

Speaking of testing, indicators are indeed precious "testing helpers." As a matter of fact it is difficult to see how theoretical terms could be empiricized and tested otherwise, that is, without having recourse to indicators. Indicators are also expedient shortcuts for the empirical checking of observational terms. Yet the question remains: Indicators of what? If we have fuzzy concepts, the fuzziness will remain as it is. That is to say that indicators cannot, in and by themselves, sharpen our concepts and relieve us from composing and decomposing them along a ladder of abstraction.

IV. COMPARATIVE FALLACIES: AN ILLUSTRATION

We may now confront in more detail how the ladder of abstraction scheme brings out the snares and the faults of our current way of handling the travelling problem of comparative politics. For we may now settle at a less rarified level of discussion and proceed on the basis of examples. It is pretty obvious that my line of analysis largely cuts across the various theories and schools that propose themselves for adoption in comparative politics, for my basic preoccupation is with the ongoing work of the "normal science," i.e., with the common conceptual problems of the discipline. Nonetheless it will be useful to enter here a somewhat self-contained illustration which bears not only on discrete concepts, but equally on a theoretical framework. I have thus selected for my first detailed discussion the categories of "structure" and "function," and this precisely on account of their crucial role in establishing the structural-functional approach in the political science setting.[43]

In introducing his pioneering comparative volume, Almond boldly asserts: "What we have done is to separate political function from political structure."[44] This separation is indeed crucial. But ten years have gone by and the assignment remains largely unfulfilled. Indeed the structural-functional school of thought is still grappling—with clear symptoms of frustration—with the preliminary difficulty of defining "function"—both taken by itself and in its relation to "structure."[45]

Whether function can be simply conceived as an "activity" performed by structures; or whether it is more proper to construe function as an "effect";[46] or whether function should be conceived only as a "relation" among structures[47]—this controversy turns out to be largely immaterial in the light of our substantive performance. That is to say, if our attention turns to the functional vocabulary in actual use, a perusal of the literature quickly reveals two things: a tantalizing anarchy (on this more later), and, second, that the functional terminology employed most of the time by most practitioners definitely carries a purposive or teleological connotation. Skillful verbal camouflage may well push the teleological implication in the background. Yet it is hard to find a functional argumentation which really escapes, in the final analysis, *Zweckrationalität,* what Max Weber called rationality of ends.[48] We may well quarrel about the definition;[49] yet the substance of the matter remains that the definitional controversy has little bearing on our subsequent proceedings. If so, it suits my purposes to settle for the way in which most people use "function" in practice (regardless of how they theorize about it), and thereby to settle for the common sense, unsophisticated meaning.

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[42] This is not to say that operationalization allows *eo ipso* for quantitative measurements, but to suggest that either operational definitions are ultimately conducive to measurement, or may not be worthwhile.

[43] I specify political science setting to avoid the unnecessary regression to Malinowski and Radcliff-Brown. This is also to explain why I set aside the contributions of Talcott Parsons and of Marion J. Levy. Flanigan and Fogelman distinguish between three major streams, labeled 1) eclectic functionalism, 2) empirical functionalism (Merton), and 3) structural-functional analysis. ("Functional Analysis," in Charlesworth, *Contemporary Political Analysis*, pp. 72–79). My discussion exclusively applies to part of the latter.

[44] Gabriel A. Almond and James S. Coleman, *The Politics of the Developing Areas* (Princeton: Princeton University Press, 1960), p. 59.

[45] It should be understood that by now the structural-functional label applies to a widely scattered group operating on premises which are largely at variance.

[46] This focus was suggested by R. K. Merton, whose concern was to separate function—defined as an "observable objective consequence"—from "subjective disposition," i.e., aims, motives and purposes (*Social Theory and Social Structure*, Glencoe: The Free Press, rev. ed., 1957, p. 24 and, *passim*, pp. 19–84.) In attempting to meet the difficulties raised by the Mertonian focus, Robert T. Holt construes functions as "sub-types" of effects, and precisely as the "system-relevant effects of structures"; understanding system-relevance as the "system-requiredness" which is determined, in turn, by the "functional requisites" of a given system. ("A Proposed Structural-Functional Framework," in Charlesworth, *Contemporary Political Analysis*, pp. 88–90). My own position is that Merton overstated his case thereby creating for his followers unnecessary and unsettled complications.

[47] This is the mathematical meaning of function. E.G. according to Fred W. Riggs in systems theory function refers to "a relation between structures." ("Some Problems with Systems Theory—The Importance of Structure," mimeographed p. 8. A redrafted version is scheduled for publication in Michael Haas and Henry Kariel (eds.), *Approaches to the Study of Political Science*, (Chandler Publishing Co.) There are problems, however, also with this definition. In particular, while the mathematical meaning of function is suited for whole systems analysis, it hardly suits the needs of segmented systems analysis.

When we say, somewhat naively, that structures "have functions," we are interested in the reason for being of structures: we are implying, that is, that structures exist *for* some end, purpose, destination or assignment.[50] This is tantamount to saying that "function" points to a means-end relationship (which becomes, from a systemic viewpoint, also a part-whole relationship), i.e., that function is the activity performed by a structure—the means—vis-à-vis its ascribed or actually served purpose.[51] Conversely, dis-function, non-functionality, and the like, indicate—from different angles—that the assigned purpose is not served by a given structure. And this current usage of function goes a long way to explain, in turn, our difficulties with structure.

The major problem with "structure" is, in fact, that political bodies and institutions largely bear, if not a functional denomination, a functional definition. Either under the sheer force of names—which is in itself a tremendous force—or for the sake of brevity, political structures are seldom adequately defined on their own terms—*qua structures.* That is to say, on the one hand, that we dispose of a functional (purposive) vocabulary, whereas we badly lack a structural (descriptive) vocabulary; and that, on the other hand, even when we deliberately ask "what is," we are invariably prompted to reply in terms of "what for." What is an election? A means (a structure) *for* electing office holders. What is a legislature? An arrangement *for* producing legislation. What is a government? A set-up *for* governing. The structure is almost invariably perceived and qualified by its salient function.[52] This makes a great deal of sense in practical politics, but represents a serious handicap for the understanding of politics.

The plain fact is, then, that the structural-functional analyst is a lame scholar. He claims to walk on two feet, but actually stands on one foot—and a bad foot at that. He cannot really visualize the interplay between "structure" and "function" because the two terms are seldom, if ever, neatly disjoined: the structure remains throughout a twin brother of its imputed functional purposes. And here we enter a somewhat vicious whirl which leads the approach to conclusions which, if true, would be self-denying.

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[48] Rationality of ends should not be confused with *Wertrationalität,* value rationality, among other reasons because in the former perspective all conceivable ends can be hypothesized as being equally valid. Hence in the *Zweckrationalität* perspective there is little point in unmasking functions as "eu-functions" or, conversely, as "caco-functions." Whether the good goals of one man are the bad goals of the next man becomes relevant only if we enter a normative, *Wertrationalität* discussion.

[49] For the many additional intricacies of the subject that I must neglect, a recent, interesting reader largely focussed on the "debate over functionalism" is N. J. Demerath and R. A. Peterson (eds.), *System, Change and Conflict* (New York: Free Press, 1967). For a critical statement of the inherent limitations of functionalism see W. C. Runciman, *Social and Political Theory* (Cambridge: Cambridge University Press, 1963), pp. 109–123. Hempel equally takes a critical view of "the logic of functional analysis" (in Gross, *Symposium on Sociological Theory,* pp. 271–307), but his standpoint is often far removed from our problems.

[50] This is not to fall prey to the subjectivistic fallacy on which Merton builds his case (*supra,* note 46). Purpose may be a "motivation" of the actor, but may equally be—as it is in teleological analysis—an "imputation" of the observer.

[51] "Unintended functions"—the fact that structures may serve ends and obtain results which were neither forseen nor desired by the structure builders—can be entered, for the economy of my argument, into the list of the purposes actually served. Likewise "latent functions" are immaterial to my point.

[52] Riggs makes the same point, namely, that "current terminology quite confusingly links structural and functional meanings" from the opposite angle that expressions such as "legislature and public administrator . . . are normally defined structurally, the first as an elected assembly, the second as a bureaucratic office"; but then goes on to say that "the words . . . also imply functions" (p. 23 of the paper cit. *supra,* note 47). It should be understood, therefore, that my "structural definition" calls for a thorough structural description. If the argument were left at defining a legislature as an elected assembly, then it can be made either way, as Riggs does.

Whatever else the structural-functional scholar may have failed to discover, he feels pretty sure about three points: first, no structure is unifunctional, i.e., performs only one function; second, the same structure can be multifunctional, i.e., can perform across different countries widely different functions; third, and therefore, the same function has structural alternatives, i.e., can be performed by very different structures. Now, to some extent these points are undeniable—but only to the extent sensed at any time by any perceptive comparative scholar. My quarrel is with the emphasis, which is unwarranted and positively misleading.

Is it really the *same* structure that functions differently? Or is the functional performance different because the structure is not the same? The thesis generally lacks adequate evidence on the structural side. For instance, "elections" are multifunctional (they may well serve the purpose of legitimizing a despot), but "free elections" are not.[53] That is to say, as soon as the electoral process obtains a structural underpinning—the minute and multiple structural conditions that make for free voting—electoral multifunctionality rapidly comes to an end. If the voter *is* offered alternatives, if the candidates *are* free to compete, if fraudulent counting *is* impossible, then free elections do serve—everywhere—the purpose of allowing an electorate to select and dismiss office holders. In view of this primary, fundamental purpose the *same* electoral structure (same in providing *all* the necessary safeties) either approaches uni-functionality, or leaves us with non-functionality, e.g., with the finding that illiterate voters are unable to use electoral mechanisms which presuppose literacy.

While the most serious problem and default is that the structures are inadequately pinpointed and described, let me hasten to add that we are not performing much better from the functional end of the argument. For our functional categories also generally lack adequate underpinning. Surprisingly enough—if one considers the far greater ease with which the functional side of the problem can be attacked—our functions tend to be as unhelpful as our structures.

For instance, if one asks, "Why a party system?" the least challengeable and most inclusive reply might be that parties perform a communication function. And if the problem is left at that, it easily follows that the authorities and the citizens "communicate," in some sense, in any polity, i.e., even when no party system exists. Hence party systems have structural alternatives—*quod erat demonstrandum.* But the problem cannot be left at that, i.e., with an unbounded, no-difference notion of communication which nullifies the problem. And the underpinning of communication brings out, first, that there is an essential difference between up-going and descending communication, and, second, that it is equally important to distinguish between "communication-information" and "communication-pressure." If so, to define a party system as an instrument for "communicating" demands and conveying "information" to the authorities, is to miss the point. A party system is, in reality, a mechanism for *sustaining* demands—and *pressing* demands—all the way through to policy implementation. What is at stake, then, is the passage from a two-way (reversible) communication-information to a prevalence of up-going communication-pressure. And for this latter purpose we have not devised, so far, any structural alternative. A party system turns out to be, therefore, a non-replaceable, *unique structure* as soon as we spell out its distinctive, crucial reason for being.

A more careful scrutiny goes to show, then, that the multi-functional, multi-structural argument has been pushed far too far, indeed to the point of becoming erroneous. Aside from the error, the irony of the situation is that, as it stands, the thesis appears self-defeating. If the same structure performs utterly different functions in different countries, and if we can always find structural alternatives for whatever function, what is the use of structural-functional analysis?

Pulling the threads together, I need not spend much time in arguing that the stalemate and the mishandlings of the structural-functional approach have a lot to do with the ladder of abstraction.

On the functional side of the coin we are encumbered by a wealth of haphazard functional categories which are merely enumerated (hardly classified according to some criterion, and even less according to the logical requirements of a taxonomical tree-type unfolding), and definitely provide no clues as to the level and type of analysis (e.g., total versus partial systems analysis) to which they apply.[54] As a result the global functional argument developed by a number of structural-functionalists remains suspended in mid-air—for lack of a coordinated medium level taxonomic support—and is left to play with overstretched, if not contentless, functional universals.

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[53] I cite the title of W. J. M. MacKenzie's book *Free Elections,* (London: Allen & Unwin, 1958) to imply that a real structural underpinning may well presuppose a hundred-page description.

[54] A sheer list of the functional denominations, roles or attributions scattered throughout the literature on political parties suffices to illustrate the point, and would be as follows: participation,

On the structural side of the coin we are confronted, instead, with little more than nothing. Structures qualified on their own right hardly exist—at least in the Almond line of thinking.[55] This is all the more regrettable in view of the fact that while functions are meant to be (at least in global comparative politics) broad explanatory categories which do not require a low level specification, structures bear, instead, a closer relation to observables, and definitely need under-pinning all the way down the ladder. With structures understood as organizational structures we are required, in fact, to descend the ladder all the way down to low level configurative-descriptive accounts.

Starting from the top, one can identify—with the help of minor terminological devices—at least four different levels of analysis: 1) structural principles (e.g., pluralism), 2) structural conditions (e.g., the class or the economic structure), 3) organizational patterns (with relation to membership systems), 4) specific organizational structures (e.g. constitutions). By saying "structural principles" I mean that as an HL category the notion of structure can only point to the principles according to which the component part of polities, or of societies, are related to each other. With reference, instead, to the low level of abstraction it should be clear that constitutions and statutes are not the "real structure. Nonetheless behavior under written rules is easier to pin down than behavior under diffuse roles, and excessive anti-formalism leads us to neglect organizational theory and the extent to which legally enforced regulations do mold behavior.

In summing up, not only has the structural-functional scholar ignored the ladder of abstraction, but he has inadvertently destroyed, during his reckless climbing, his own ladder.[56] So much so that the approach encounters exactly the same perplexity as, say, general systems theory, namely, "Why has no scholar succeeded in presenting a structural-functional formulation which meets the requirements of empirical analysis."[57] Now, it is hardly surprising that the general systems theorist should encounter great difficulties in deriving testable propositions about politics, since he is required to proceed deductively on the basis of theoretical primitives.[58] But this is not the case with the structural-functional approach, which is not necessarily committed to whole systems analysis and enjoys the distinctive empirical advantage of leaning extensively—especially with segmented systems analysis—on observational terms.[59] So, why should the struc-

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electioneering, mobilization, extraction, regulation, control, integration, cohesive function, moderating function, consensus maintenance, simplification of alternatives, reconciliation, adaptation, aggregation, mediation, conflict resolution, brokerage, recruitment, policy making, expression, communication, linkage, channelment, conversion, legitimizing function, democratization, labelling function.

[55] I make specific reference to Almond because I believe that his very conception of structure is largely responsible for this outcome. For instance, "By structure we mean the observable activities which make up the political system. To refer to these activities as having a structure simply implies that there is a certain regularity to them." (Almond and Powell, *Comparative Politics: A Developmental Approach,* Boston: Little, Brown, 1966, p. 21). In the subsequent paragraph one reads: "We refer to particular sets of roles which are related to one another as *structures.*" Under such porous and excessively sociological criteria, "structure" becomes evanescent.

[56] This complaint is ad hoc, but could be expanded at length. On the general lack of logical and methodological status of the approach two strong critical statements are: R. E. Dowse, "A Functionalist's Logic," *World Politics,* (July 1966), 607-622; and A. L. Kalleberg, "The Logic of Comparison," *World Politics,* 18 (October 1966), 69-82. While the two authors are overconscious thinkers, I would certainly agree with Dowse's concluding sentence, namely, that "to ignore trivial logical points is to risk being not even trivially true" (p. 622).

[57] Flanigan and Fogelman in Charlesworth, *Contemporary Political Analysis* pp. 82-83.

[58] On general systems theory one may usefully consult Oran R. Young, *Systems of Political Science* (Englewood Cliffs: Prentice-Hall, 1968), Chap. 2. See also Giuliano Urbani, "General Systems Theory: Un Nuovo Strumento per l'Analisi dei Sistemi Politici?," *Il Politico,* 4 (1968), 795-819.

[59] While there is some controversy on the respective merits and shortcomings of the two strategies, the structural-functional approach is not inherently tied to either one. For the partial versus whole systems controversy the two stances are well represented by J. LaPalombara, who favors the segmented approach. (cf. esp. "Parsimony and Empiricism in Comparative Politics: An Anti-Scholastic View," in R. T. Holt and J. E. Turner (eds.), *The Methodology of Comparative Re-*

tural-functional scholar remain tied to "a level of analysis which [does] not permit empirical testing?"[60] According to my diagnosis there is no intrinsic reason for this. Quite to the contrary, we may expect very rewarding returns, and the empirical promise (and distinctiveness) of the approach may well near fulfillment, if we only learn how to maneuver along a ladder of abstraction.

Let us now pass on to a more loose discussion —the second part of this illustration—for which I have selected a somewhat different family of categories: pluralism, integration, participation and mobilization.[61] While one may think of many other examples that would suit my purposes just as well, the four categories in question are representative in that they are used for significant theoretical developments not only under a variety of different frameworks, but also by the non-affiliated scholar, thereby including—in the case of participation and mobilization—the scholar who happens to be interested only in statistical manipulations.

Given the fact that pluralism, integration, participation and mobilization are culture-bound concepts which may reflect—as far as we know at the outset—a distinctive Western experience, the methodological caveat here is that the reference area should make for the starting point of the investigation. So to speak, we are required to elaborate our culture-bound concepts in a "we-they" clockwise direction. It is proper, therefore, to start with the question: How do we understand pluralism, integration, participation and mobilization in their domestic, original context?

At home "pluralism" does not apply to societal and/or political structure, nor to interplay between a plurality of actors. Pluralism came to be used, in the Western literature, to convey the idea that a pluralistic society is a society whose structural configuration is shaped by pluralistic beliefs, namely, that all kinds of autonomous subunits should develop at all levels, that interests are recognized in their legitimate diversity, and that dissent, not unanimity, represents the basis of civility. Pluralism is indeed—as already noted —a highly abstract structural *principle*. Yet the term points to a *particular* societal structure— not merely to a developed stage of differentiation and specialization—and does retain a wealth of characterizing connotations whenever we discuss, in the Western democracies, our internal policies and problems.

"Integration" can be conceived as an end-state, as a process, or as a function performed by integrating agencies (parties, interest groups, etc.). In any case, in the Western polities integration is not applied to *whatever* kind of "putting together," to *whatever* state of amalgamation. For instance, when American scholars discuss their own domestic problems, they have definite ideas of what is, and what is not, integration. They would deny that integration has anything to do with "enforcing uniformity." They are likely to assume, instead, that integration both presupposes and generates a pluralistic society (as qualified above). And, surely, an integrative agency is required to obtain a maximum of coalescence and solidarity with a minimum of coercion.[62]

Similar points can be made with regard to participation and mobilization. Regardless of whether "participation" is used normatively (as pointing to a basic tenet of the democratic ideal) or descriptively (as reflecting a democratic experience), in either case in our domestic discussions participation is not any such kind of "taking part." Thus the advocates of a participatory democracy are hardly satisfied by any kind of involvement in politics. To them participation means *self-motion;* it does not mean being manipulated or coerced into motion. And surely the original definite meaning of the term conveys the idea of a free citizen who acts and intervenes—if he so wishes—according to *his* best judgement. So conceived, participation is the very opposite, or the very reverse, of mobilization. Mobilization does not convey the idea of individual self-motion, but the idea of a malleable, passive collectivity which is being *put into motion* at the whim of persuasive—and more than persuasive—authorities. We say that indi-

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search, op. cit., pp. 125–149); and, for the contrary view, Fred W. Riggs (cf. especially his forthcoming essay in Haas and Kariel, *Approaches to the Study of Political Science.*)

[60] Flanigan and Fogelman, *op. cit.*

[61] The relevant "family difference' is that structure and function are not culture-bound concepts, while the four other categories are. This is also to note that the travelling problem of comparative politics cannot be reduced to the construction of "non-culture bound" conceptualizations. How to use those conceptualizations which cannot help being culture bound is equally a problem.

[62] Since we are discussing here macro-problems and macro-theory I need not follow the concepts under investigation all the way down the ladder of abstraction. I should not let pass unnoticed, however, that "integration" also belongs to the vocabulary of sociology and psychology, thereby lending itself to very fine lower level distinctions. See e.g., W. S. Landecker, "Types of Integration and their Measurements," in *The Language of Social Research, op. cit.,* pp. 19–27.

viduals "participate," but we cannot say about the same individuals that they "mobilize"—they *are mobilized.*

It is quite clear, then, that pluralism, integration, participation and mobilization all have specific connotations which can be pinned down, and are in fact retained—no matter how implicitly—in our Western enquiries and controversies. However, in the context of global comparative politics the specificity of these notions gets lost: there is no end to pluralism; integration is applied indifferently to pluralistic and non-pluralistic settings; and participation and mobilization are turned into largely overlapping notions. There is no end to pluralism, for we are never told what is non-pluralism. Since pluralism exists somewhere, the assumption appears to be that "to a different degree" pluralism will be found to exist everywhere. However, a different degree *of what?* This is indeed the irony of using a degree language—intended when used appropriately to convey precision—for conveying elusiveness. Likewise the meaning of integration changes, and eventually evaporates, en route. Finally, and similarly, the distinction between participation and mobilization only holds at home. With most comparative oriented scholars mobilization comes to mean whatever process of social activation; and participation is currently applied by the discipline at large both to democratic and mobilizational techniques of political activation.

At this stage of the argument I need not labor at explaining why and how we obtain these drastic losses of specifity. They result, as we know, from conceptual stretching, which results, in turn, from incorrect ladder climbing: the clumsy attempt to arrive at "travelling universals" at the expense of precision, instead of at the expense of connotation (i.e., by reducing the number of qualifying attributes). What remains to be highlighted are the consequences of this state of affairs.

Take, for instance, the formidable errors in interpretation and prediction which are suggested by the universal, unspecified application of "pluralism" and "integration." If we say that African societies are not pluralistic but "tribalistic," the argument is likely to be that a situation of tribalistic fragmentation hardly provides the structural basis not only for integrative processes to occur, but also for bringing integrative agencies to the fore. Indeed my argument would be that the functional needs, or feedbacks, of a fragmented society are at odds with the functional feedbacks, or needs, of a pluralistic society. In Europe, for instance, medieval fragmentation generated monarchical absolutism. However, if pluralism is vaporized into an empty generality, then we are authorized to call African societies pluralistic, and the unfortunate implication may well be that we expect Africans to solve their problems as if they had to deal with Western-type societies.[63]

"Mobilization" is also a worthwhile example in that it confronts us with a problem that has only been mentioned, so far, in passing. While pluralism, integration and participation are derived from our experience with democracy—i.e., from the context of the democratic polities—we also dispose of a limited set of terms which originate from a totalitarian context. This is the case of the term mobilization, which derives from military terminology—especially the German total mobilization of World War 1—and enters the vocabulary of politics via the militia type of party (as Duverger calls it), and specifically via the experience of fascism and of nazism.[64] Nonetheless the term is currently applied also to the democratic polities—and this means that we have drawn a "reversed extrapolation" (i.e., a counter-clockwise extrapolation). And since we often complain that our terminology is democracy-centered, my first complaint is that we fail to take advantage of the fact that we do have terms which escape the democratic bias. However, the inconvenience resulting from reversed extrapolations are seen best on a broader scale, and with particular reference to what I call the "boomerang effect" of the developing areas.[65]

Western scholars travelling across Africa or South-East Asia discover that our categories hardly apply, which is hardly surprising. From this they conclude—and this is the boomerang effect—that the Western categories also should not be applied to the West. But this is a strange inference. Granted that global comparative politics requires minimal common denominators, it does not follow that we should escape Western parochialism by masquerading in non-Western clothes. For one thing, it may well be that a number of ancient civilizations appear diffuse and amorphous to the Western observer precisely because he lacks the categories for coping with devious, overly sedimented, "non-rational" structural patterns. On the other hand, and assuming that underdeveloped political societies may be far less structured than others, this is no reas n for feeding back shapelessness where structural differentiation does exist. Hence, reversed extrapolations are a fallacy, and the problem of establishing a minimal common denominator does not authorize us to feed primitivism and formlessness into non-primitive settings.

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[63] The point could be extended at great length. E.g., I would assume that only in a truly pluralistic society (i.e., qualified by the characteristics conveyed by the Western use of term) may differentiation result in, and join forces with, integration. But much of the literature on political development seems to miss this essential condition.

[64] Shils and Deutsch relate the notion also to Mannheim's "fundamental democratization" (see esp. K. W. Deutsch "Social Mobilization and Political Development," this REVIEW, 55, September 1961, p. 494). But while Mannheim may well have provided the bridge across which "mobilization" entered the vocabulary of democracy, the fact remains that the term was commonly used in the early thirties, in Italy and in Germany, as reflecting a distinctly totalitarian experience.

[65] The boomerang effect is also responsible, in part, for the disappearance of politics (*supra,* note 5).

If I may generalize from the foregoing, it appears that much of the ongoing work of the discipline is plagued by "meaningless togetherness," and thereby by dangerous equivocations and distortions. In particular, and especially important, under these conditions we are dangerously exposed to "begging the question," i.e., to assuming what we should be proving: the *petitio principii* fallacy. For instance, if "mobilization" is applied to a democratic polity the suggestion is that democracies mobilize more or less as totalitarian regimes do. Conversely, if "participation" is applied to a totalitarian system the suggestion is that democratic participation also occurs, to some extent at least, in nondemocratic settings. Now this may well be the case. But the case cannot be proven by *transferring the same denomination* from one context to another. For this amounts to pure and simple terminological camouflage: things are declared alike by making them *verbally* identical.

All in all, then, it can hardly be held that our "losses of specificity" are compensated by gains in inclusiveness. I would rather say that our gains in travelling capacity, or in universal inclusiveness, are verbal (and deceptive) while our "gains in obfuscation" are very substantial.

I cannot discuss this further. As LaPalombara vividly puts it, "so many of our generalizations about the political process move with apparent randomness from the micro to the macroanalytic levels"—the result being "messiness caused by confusion as to the level of analysis."[66] Following this line of complaint I have argued that confusion as to the level of analysis brings about these unfortunate results: 1) at the higher levels, macroscopic errors of interpretation, explanation and prediction; 2) at the lower levels, a great deal of wasteful data misgathering; 3) at all levels, confusion of meaning and destruction of the sharpness of our concepts. We do lack words. But conceptual stretching and poor logic have largely impoverished the analytical articulation and the discriminating power of the words that we do have. And my feeling is that only too often major differences are being cancelled on the thin basis of secondary, trivial similarities. It would hardly make sense to say that men and fishes are alike in that both classes share a "swimming capability." Yet much of what we are saying in global comparative politics may not make much more sense.

Let me stress, to conclude, that according to my scheme of analysis all of this is unnecessary. Awareness of the ladder of abstraction shows that the need for highly abstract, all-embracing categories does not require us to inflate, indeed to evaporate, the observational, empirically-linkable, categories that we do have. Moreover, if we know how to climb and descend along a ladder of abstraction—and thereby know where we stand in relation to the "property space" of the analysis that we are pursuing—not only conceptual stretching is ruled out, but also faulty analogies and the begging-the-question fallacy can be disposed of.

V. SUMMARY

Especially during the last decade comparative politics as a substantive field has been rapidly expanding. The scale, if not the scope, of this expansion raises thorny and unprecedented problems of method. But we seem to embark more and more in comparative endeavors without *comparative method,* i.e., with inadequate methodological awareness and less than adequate logical skills. That is to say, we seem to be particularly naive vis-à-vis the logical requirements of a world-wide comparative treatment of political science issues.

My focus is conceptual—about concepts—under the assumption that concepts are not only elements of a theoretical system, but equally tools for fact-gathering, data containers. The empirical problem is that we badly need information which is sufficiently precise to be meaningfully comparable. Hence we need a filing system provided by discriminating, i.e., taxonomic, conceptual containers. If these are not provided, data misgathering is inevitable; and statistical, computerized sophistication is no remedy for misinformation. The theoretical problem can be stated, in turn, as follows: we grievously lack a disciplined use of terms and procedures of comparison. This discipline can be provided, I suggest, by awareness of the ladder of abstraction, of the logical properties that are implied, and of the rules of composition and decomposition thus resulting. If no such discipline is obtained, conceptual mishandling and, ultimately, conceptual misformation is inevitable (and joins forces with data misgathering).

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[66] *Comparative Politics* (October 1968), p. 72.

Thus far the discipline has largely followed the line of least resistance, namely, "conceptual stretching." In order to obtain a world-wide applicability the extension of our concepts has been broadened by obfuscating their connotation. As a result the very purpose of comparing—control—is defeated, and we are left to swim in a sea of empirical and theoretical messiness. Intolerably blunted conceptual tools are conducive, on the one hand, to wasteful if not misleading research, and, on the other hand, to a meaningless togetherness based on pseudo-equivalences.

The remedy resides—I submit—in our combined ability 1) to develop the discipline along a medium level of abstraction with better intermediate categories, and 2) to maneuver, both upwards and downwards, along a ladder of abstraction in such a way as to bring together assimilation and differentiation, a relatively high explanatory power and a relatively precise descriptive content, macro-theory and empirical testing. To be sure, no level of analysis can be exactly translated and converted into the next level. In this sense, something is always lost (and gained) along the ladder. But a disciplined use of terms and procedures of comparison generates, at each level, sets of propositions which either reinforce or contradict the propositions of the neighboring levels.

The suggestion has recently been put forward that "political scientists turn to mathematics for [the] rules of logic" required "to introduce the necessary deductive power into a paradigm."[67] I have taken the more sober, and indeed counter-perfectionistic view that we should not encourage the "overconscious thinker" paralyzed by overly ambitious standards. But surely we cannot expect an unconscious thinker lacking elementary logical training and discipline to meet the intricate new problems arising from global comparisons.

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[67] Holt and Richardson, "Competing Paradigms in Comparative Politics," in Holt and Turner, *The Methodology of Comparative Research,* cit., p. 70. The chapter is perhaps perfectionistic, but surely a very intelligent and stimulating "stock taking" overview.

PERSONALITY AND CONFORMITY: THE LEARNING OF POLITICAL ATTITUDES*

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Why do some men embrace society's values while others reject them? Is conformity a general trait, more uniformly manifested by some people than by others? What social or psychological forces lie behind the tendency to conform or deviate?

Although these questions obviously have significance for the conduct of political life, they have received far less attention from political scientists than from scholars in other disciplines such as psychology and sociology. In view of current challenges to the legitimacy of existing political institutions, the mounting debate over the acceptable limits of protest, and the growing disdain for democratic decision-processes shown by some segments of the population, the need for political scientists to understand the nature and sources of conformity and deviation has become, if anything, more urgent. We hope, in the present paper, to explore the psychological—and to some extent the social and political—meaning of conformity and deviation as reflected in citizen responses to political beliefs. To that end we shall review briefly the present state of psychological theory and research on conformity behavior; suggest, in light of our own research findings, some ways in which current psychological explanations might be modified and extended to account for conformity and deviation within the mass public; and furnish data that might help to explain why individuals who have different personality characteristics and who occupy different roles in the society are likely to accept or reject political norms.

Until recently, most writers who dealt with conformity and deviation were concerned to evaluate them morally rather than to investigate them scientifically. Discursive treatments of the subject tend even today either to reify these concepts or to treat them as moral universals, appraising them as good or bad according to the particular bias of the author. Rarely is it recognized in such discussions that conformity and deviation are terms of relationship that take their meaning from the contexts in which they occur and from the combination of forces that happens in the particular instance to give rise to them. In practice, of course, everyone is to some degree both conformer and deviant, adhering to certain community and group standards but not to others.

Not all acts of conformity spring from the same motivations. Some arise from a tendency to yield to intimidation and some from hunger for approval and group solidarity; others, however, result from the ubiquitous human need to reduce moral and perceptual ambiguity, and still others from a rational assessment of the social realities and a recognition that accommodation to society's standards can help to bring about mutually desired goals. Thus, conformity to social norms may only occasionally signify deference, servility, obsequious surrender, or weakness of character—just as deviation or resistance to norms may only occasionally represent a superior conscience, personal independence, or courage to defy Establishment pressures. In short, conformity and deviation are descriptive terms that merely denote one's response to norms; they can be morally appraised only with reference to their particular motivation and consequences. We shall also see that conformity—defined as the acceptance of majority beliefs—is not necessarily the same as gullibility, yea-saying, suggestibility, or even, for that matter, persuasibility.

Whether desirable or undesirable, conformity to community and group standards is, in any event, "natural." Few social science findings have been more firmly and repeatedly confirmed than those revealing man's vulnerability to group pressure and his responsiveness to group norms.[1] Through association with family, peers,

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* This is publication A108 of the Survey Research Center, University of California, Berkeley. The research was conducted under grants to Herbert McClosky from the Social Science Research Council and supported in part by Public Health Service Research Grant MH-05837, from the National Institutes of Health. David Koff assisted in the planning and tabulation stages of the project, and Ellen Siegelman offered her valuable advice at the later stages. We owe a special debt to Paul Sniderman for his many penetrating suggestions concerning both the intellectual content and editorial style of the manuscript.

[1] For reviews of the research see especially William J. McGuire, "Personality and Susceptibility to Social Influence," in E. F. Borgatta and W. W. Lambert (eds.), *Handbook of Personality Theory and Research*, (Chicago: Rand McNally, 1968), pp. 1130–1187; William J. McGuire, "The Nature of Attitudes and Attitude Change," in Gardner Lindzey and Elliot Aronson (eds.), *Handbook of Social Psychology*, 2nd ed. (Reading, Mass.: Addison-Wesley, 1968), Vol. 3, Chap. 21; Vernon L. Allen,

and other reference groups, and through the routine mechanisms of socialization (e.g., imitation, modelling, indoctrination, reinforcement), one learns which standards society prizes and one conforms to them, for the most part, without reflection.

How effectively these values are transmitted will partly depend on one's proximity to the cultural mainstream and partly on one's individual personality characteristics. A mounting body of experimental and other evidence has shown that not everyone is equally susceptible to group pressure or persuasion.[2] It is with these differences—particularly the relation of personality differences to the adoption of social norms—that we are mainly concerned in this paper. By examining the data collected in two field surveys, we hope to show how personality variables affect the acceptance or rejection of community beliefs. In addressing this question, we also hope to gain further insight into the nature of conformity and deviation in natural settings.

I. THEORY AND PREVIOUS RESEARCH

The most prolific and systematic research on susceptibility to influence has been carried on by the psychologists, chiefly through small group experiments. Three major experimental traditions have evolved—those of conformity, persuasibility, and suggestibility.[3] In the typical *conformity* (or convergence) experiment, psychological pressure is exerted upon a subject to induce him to yield to a group standard. No effort is made, however, to win him over by persuasive arguments; instead, he is confronted by group opinions or perceptions discrepant with his own. The measure of his conformity is the distance he moves toward the position held by the group.[4] Although variations occur in the type of stimulus or group structure employed, almost all conformity experiments aim to assess the subject's susceptibility to group influence. In the typical *persuasibility* (or attitude change) experiment, the subject is exposed to arguments and communications deliberately designed to persuade him to shift his beliefs in the direction of the message.[5] Here too the experiments vary in detail: in some the source of the communication is revealed, in others concealed; in some the source is congenial, in others inimical. The message employed may be threatening or reassuring; it may be delivered in written form, or via recording, film, or face-to-face communication. Some persuasibility studies are conducted in the laboratory, and some in actual field situations. Studies of *suggestibility* (which are least relevant for our purposes) have mainly been concerned with motor and sensory responses to influence inductions, focusing upon such phenomena as body sway, odor suggestibility, sensory hallucinations, and hypnotizability.[6]

Research involving these concepts has yielded an abundant, complex, but not altogether consistent set of findings. In general, however, the studies show that personality and cognitive characteristics affect conformity and deviation in important ways. For example, in conformity

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"Situational Factors in Conformity," in Leonard Berkowitz (ed.), *Advances in Experimental Social Psychology,* Vol. 2 (New York: Academic Press, 1965), pp. 133–176; I. A. Berg and B. M. Bass (eds), *Conformity and Deviation,* (New York: Harper, 1962); Arthur R. Cohen, *Attitude Change and Social Influence,* (New York: Basic Books, 1964); Carl I. Hovland and Irving L. Janis (eds.), *Personality and Persuasibility,* (New Haven: Yale University Press, 1959). See also E. P. Hollander and R. H. Willis, "Some Current Issues in the Psychology of Conformity and Non-Conformity," *Psychological Bulletin,* 68 (1967), 62–76.

[2] See, for example, Solomon E. Asch, "Effects of Group Pressure Upon the Modification and Distortion of Judgments," in Harold Guetzkow (ed.), *Groups, Leadership and Men,* (Pittsburgh: Carnegie Press, 1951), pp. 177–190; McGuire, "Personality and Susceptibility," *loc. cit.;* Hovland and Janis, *Personality and Persuasibility;* Richard S. Crutchfield, "Conformity and Character," *American Psychologist,* 10 (1955), 191–198; Frank Barron, "Some Personality Correlates of Independence of Judgment," *Journal of Personality,* 21 (March, 1953), 287–297.

[3] Cf. McGuire, "Personality and Susceptibility," *op. cit.,* pp. 1131–1136.

[4] Leading examples of this experimental tradition are Solomon E. Asch, "Studies in Independence and Conformity: I. A Minority of One against a Unanimous Majority," *Psychological Monographs,* 70 (No. 9, 1956); Muzafer Sherif, "Group Influence upon the Formation of Norms and Attitudes," in Eleanor E. Maccoby *et al.* (eds.), *Readings in Social Psychology,* 3rd ed., (New York: Holt, Rinehart, and Winston, 1958), pp. 219–232; Crutchfield, *op. cit.*

[5] The major research in this field has been conducted by the "Yale School" under the direction of Carl I. Hovland. Three illustrative publications are Carl I. Hovland *et. al., Communication and Persuasion* (New Haven: Yale University Press, 1953); Carl I. Hovland *et al., The Order of Presentation in Persuasion,* (New Haven: Yale University Press, 1950); Hovland and Janis, *Personality and Persuasibility.*

[6] E. R. Hilgard, *Hypnotic Susceptibility,* (New York: Harcourt, Brace, 1965); McGuire, *loc. cit.*

experiments of the type pioneered by Asch and Crutchfield,[7] the subjects most likely to yield to group influence are those who are least well integrated psychologically and hence most poorly equipped to interact effectively with others. Subjects possessing superior intellect and creativity submit to misleading majority judgments less often than others do. The ability to withstand pressure is also related to ego-strength and self-awareness: Those who resist erroneous majority judgments usually have greater self-esteem than those who yield to them.[8] Presumably they are, among other things, less apprehensive about social disapproval.

Other personality characteristics often correlated with yielding in the conformity experiments include rigidity, dogmatism, conventionality, and authoritarianism.[9] Individuals characterized by these traits prove especially vulnerable in stressful or unpredictable situations. Their need for certitude may incline them to be negativistic and unmovable when *they* are in command, but submissive and receptive to influence when *others* wield the power and when compliance rather than resistance is the more likely to produce order and predictability. Compared with the Independents, the Yielders are also found to be less trusting and outgoing toward others. Although they crave social approval, they are less tolerant of those who disagree with them.[10] They are less poised than the Independents and have more poorly developed social skills. They generally score lower on dominance, responsibility, and achievement orientation. In short, while the Yielders may seem on the surface to be cooperative and even submissive, clinical measures reveal them to be comparatively hostile and socially maladapted.

Some of these traits also turn up as correlates of *persuasibility* in the research on attitude change and communication conducted by Hovland and his associates. Here the picture is even more complicated, the correlations lower, and the results more inconclusive. As in the conformity experiments, subjects with low self-esteem are more susceptible to persuasion and more likely to alter their opinions in the direction of the influence.[11] But even this relationship is confounded, for when the message is highly complicated or threatening, these subjects are more likely to resist persuasion and to retain the views they initially held.[12] Conclusions about the relationship between persuasibility and such personality attributes as aggressiveness, authoritarianism, and social isolation are still more equivocal. Correlations are found in some experiments but not in others, depending upon the types of participants, procedures, and measures used in the study.[13] Although Hovland and others believe that persuasibility is a general trait (i.e., a predisposition "reflecting an individual's susceptibility to influence from many different sources, on a wide variety of topics, and irrespective of the media employed"),[14] its relation to other motivational predispositions appears, from present evidence, to be tenuous and complex.

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[7] See also R. D. Tuddenham, "Correlates of Yielding to a Distorted Group Norm," *Journal of Personality,* 27 (1959), pp. 272–284; Francis Di Vesta and D. Cox, "Some Dispositional Correlates of Conformity Behavior," *Journal of Social Psychology,* 52 (1960), pp. 259–268; Crutchfield, "Conformity and Character," *op. cit.;* Crutchfield, "Detrimental Effects of Conformity Pressures on Creative Thinking," *Psychologische Beitrage,* 6 (1962), 463–471; Crutchfield, "Independent Thought in a Conformist's World," 1962, mimeographed; Crutchfield, "Personal and Situational Factors in Conformity to Group Pressure," 1957, mimeographed; Frank Barron, "The Psychology of Imagination," *Scientific American* (September, 1958).

[8] Crutchfield, "Conformity and Character," *loc. cit.;* Barron, "The Psychology of Imagination," *loc. cit.;* Tuddenham, "Correlates of Yielding," *loc. cit.;* M. L. Hoffman, "Some Psychodynamic Factors in Compulsive Conformity," *Journal of Abnormal and Social Psychology,* 48 (1953), 383–393; H. C. Kelman, "Effects of Success and Failure on Suggestibility in the Autokinetic Situation," 45 (1950), 267–285; Di Vesta and Cox, *op. cit.;* Leo Levy, "A Study of Some Personality Attributes of Independents and Conformers," *Diss. Abst.,* 19 (1959), p. 1823.

[9] Crutchfield, "Conformity and Character;" Tuddenham, "Correlates of Yielding;" Hoffman, "Some Psychodynamic Factors;" Halla Beloff, "Two Forms of Social Conformity: Acquiescence and Conventionality," *Journal of Abnormal and Social Psychology,* 56 (1958), 99–104; W. Wells *et al,* "Conformity Pressure and Authoritarian Personality," *Journal of Psychology,* 42 (1956), 133–136; David Marlow, "Some Personality and Behavioral Correlates of Conformity," *Diss. Abst.,* 20 (1959), 2388–2399.

[10] Di Vesta and Cox, *op. cit.;* Marlow, *op. cit.;* Kelman, *op. cit.;* Crutchfield, "Conformity and Character;" Leo Levy, *op. cit.;* K. R. Hardy, "Determinants of Conformity and Attitude Change," *Journal of Abnormal and Social Psychology,* 54 (1957), 289–294; H. B. Linton, "Dependence on External Influence: Correlates in Perception, Attitudes and Judgment," *Journal of Abnormal and Social Psychology,* 51 (1955), 502–507; D. W. Bray, "The Prediction of Behavior from Two Attitude Scales," *Journal of Abnormal and Social Psychology,* 45 (1950), 64–84; J. S. Mouton *et al.,* "The Relationship Between Frequency of Yielding and the Disclosure of Personal Identity," *Journal of Personality,* 24 (1956), 339–347; Tuddenham, *op. cit.*

[11] Irving L. Janis, "Personality Correlates of Susceptibility to Persuasion," *Journal of Personality,* 22 (1954), 504–518; Irving L. Janis, "Anxiety Indices Related to Susceptibility to Persuasion," *Journal of Abnormal and Social Psychology,* 51 (1955), 663–667; Irving L. Janis and Donald Rife, "Persuasibility and Emotional Disorder," in Carl I. Hovland and Irving L. Janis (eds.), *Personality and Persuasibility,* Chap. 6; Arthur R. Cohen, "Some Implications of Self-Esteem for Social Influence," *ibid.,* Chap. 5.

One need not look far to find the reasons for these uncertainties: the act of yielding to, or resisting, influence is never simple and can rarely be understood by reference to any single explanatory variable. In order to conform, an individual must be located where he can receive the message, and he must have enough interest and cognitive skill to comprehend it. His judgment will be swayed by his estimate of the source's trustworthiness, prestige, and authority, and (where content is relevant) by his feelings about the substance of the communication. He will also be affected by the context, e.g., whether the task is easy or difficult; whether clues are furnished that indicate how he is expected to respond; whether he is called upon to undergo a genuine, enduring shift in viewpoint or merely to comply publicly; whether the group to which he must respond is unanimous or divided in its judgments; and whether he has reason to feel psychologically "threatened" if he fails to comply.

Nor is the relation between conformity and a given personality trait invariably monotonic.[15] Interaction effects are common: a strong personality need may affect one's susceptibility to influence far differently than would a milder version of the same need. For example, anxiety in moderate degree often serves to motivate learning (and hence may increase persuasibility), whereas severe anxiety can be so crippling as to impede learning. A person low in self-esteem may readily open his mind to reassuring messages but close it to threatening ones, whereas a person high in self-esteem may be accessible to both. Sometimes an individual's personality traits work at cross purposes in shaping his response to influence: one trait (say low self-esteem) may lead him to yield, but another (say personality disorganization) may make it difficult for him to detect accurately what is being communicated and which influence to yield to.

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[12] A. F. Gollob and James E. Dittes, "Effects of Manipulated Self-Esteem on Persuasibility Depending on Threat and Complexity of Communication," *Journal of Personality and Social Psychology,* 2 (1965), 195–201; Richard Nisbett and Andrew Gordon, "Self-Esteem and Susceptibility to Social Influence," *Journal of Personality and Social Psychology,* 5 (1967), 268–276; Owen Silverman, Leroy H. Ford, Jr., and John B. Morganti, "Inter-related Effects of Social Desirability, Sex, Self-Esteem, and Complexity of Argument on Persuasibility," *Journal of Personality,* 34 (1966), 555–568; Leventhal and Perloe, "A Relationship between Self-Esteem and Persuasibility," *Journal of Abnormal and Social Psychology,* 64 (1962), 385–388; James M. Dabbs, Jr., "Self-Esteem, Communicator Characteristics and Attitude Change," *Journal of Abnormal and Social Psychology,* 69 (1964), 173–181; Homer H. Johnson, James M. Torcivia, and Mary Ann Poprick, "Effects of Source Credibility on a Relationship between Authoritarianism and Attitude Change," *Journal of Personality and Social Psycology,* 9 (1968), 179–183.

[13] For a discussion of some of the inconclusive and contradictory results see McGuire, "Personality and Susceptibility," *loc. cit.;* Hovland and Janis, *Personality and Persuasibility,* Chap. 11. See also Mortimer H. Appley and George Moeller, "Conformity Behavior and Personality Variables in College Women," *Journal of Abnormal and Social Psychology,* 66 (1963), 284–290; Richard D. Tuddenham, "Studies in Conformity and Yielding: II. The Influences Upon a Judgment of a Grossly Distorted Norm," Technical Report No. 2, 1957, University of California, Berkeley, N. R. 170–259; Halla Beloff, *op. cit.;* N. S. Endler, "Conformity Analyzed and Related to Personality," *Journal of Social Psychology,* 53 (1961), 271–283; Ivan D. Steiner, "Personality and the Resolution of Interpersonal Disagreements," in B. Maher (ed.), *Progress in Experimental Personality Research,* Vol. 3, (New York: Academic Press, 1966), pp. 195–239; Homer H. Johnson *et al., op. cit.;* Homer H. Johnson and Ivan D. Steiner, "Some Effects of Discrepancy Levels on Relationships between Authoritarianism and Conformity," *Journal of Social Psychology,* 73 (1967), pp. 199–205; Ivan D. Steiner and Joseph S. Vannoy, "Personality Correlates of Two Types of Conformity Behavior," *Journal of Personality and Social Psychology,* 4 (1964), 307–315; Ralph Barocas and Leon Gorlow, "Self-Report, Personality Measurement, and Conformity Behavior," *Journal of Social Psychology,* 71 (1967), 227–234.

[14] Hovland and Janis, *Personality and Persuasibility,* p. 225.

[15] William J. McGuire, "Personality and Susceptibility," see especially pp. 1143–1148.

Another complication is that susceptibility to influence often depends upon learning history and reinforcement as well as upon personality characteristics.[16] The rewards and punishments available in any given situation can strongly determine whether the behavior elicited will be deviant or conformist. Many who belong to and are reinforced by "deviant" sub-cultures will diverge from society's norms regardless of their personality needs; others will, for similar reasons, conform.

A further complication is that a given influence attempt may have different consequences in different settings—for example, in a laboratory as compared with a natural setting. These settings affect both the credibility of the rewards and punishments one encounters and the earnestness with which one confronts the research task. They also determine the number and types of communications one receives. In most laboratory experiments, the participants are treated not as individuals but as experimental *subjects* —which means either that they are all treated interchangeably or that their treatment is determined solely by their preassigned role in the experiment; the differences in their individual *personality* configurations are usually ignored. Outside the laboratory, however, people are treated differentially according to their status and their particular cognitive and personality characteristics. Some people will thus be exposed to more frequent, more varied, and more intense communications than others. Obviously, these differences in treatment and exposure will determine how often one encounters the beliefs circulating in the society, and how effectively one learns whether to embrace or reject a given standard.

The intimacy and relatively uncomplicated structure of most conformity experiments also set them apart from natural settings in ways that may be critical for the learning of norms. In the laboratory the task is usually simple, the stimulus is difficult to ignore, the direction of the group pressure is unmistakable, and the extent to which one's views are shared by the other participants is readily ascertained. In the larger society, by contrast, such matters are more difficult to decipher. Many who deviate from conventional standards are not aware that they are doing so. Confronted by a bewildering array of opinions, values, attitudes, assertions, etc., they are often unable to recognize which standards the society approves and which ones it disapproves. Their interactions with others, about public questions are often so random and casual that they have little sense of being "pressured" from *any* direction. While laboratory experiments on influence characteristically compel them to face alternatives and to make choices, a large, complex society permits them to ignore or remain neutral on many questions. The society also furnishes an extraordinary variety of groups in which individuals with deviant or unpopular outlooks can find a home, enabling them to believe that theirs is the "true" or accepted standard. Thus, the inducements to conform to or deviate from group norms can be dramatically different in the two contexts.[17]

A final complication grows out of the differences in the several types of susceptibility to influence. Experimental findings indicate that the intercorrelations among suggestibility, conformity (in the Asch sense), and persuasibility are weak. While the three concepts superficially resemble one another and are sometimes used interchangeably by investigators, they may spring from very different personality dynamics and they often describe very different forms of behavior. For example, a subject with sufficient intellectual discernment to yield to a reasoned argument in a persuasibility experiment is likely to be sufficiently perspicacious to resist the erroneous majority judgments in a standard conformity experiment.[18] Many people adhere strongly to group norms without being unusually susceptible to persuasion. The two, in fact, commonly work in opposite directions, e.g., the stronger one's initial loyalty to a group's norms, the more one resists efforts to persuade him to deviate from those norms.[19]

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[16] See for example, Norman S. Endler and Elizabeth Hoy, "Conformity as Related to Reinforcement and Social Pressure," *Journal of Personality and Social Psychology,* 7 (1967), 197–202.

[17] We are not, of course, claiming that the laboratory context is unreal, synthetic, or in any other way inappropriate to the study of conformity and persuasibility. We are saying, rather, that the findings turned up in a laboratory context, however valid for *that* context, may not take the same form or hold with the same force in some situations encountered in the larger society. As we shall see, the same principles can apply in both contexts, but manifest themselves quite differently.

[18] H. C. Kelman, "Compliance, Identification, and Internalization: Three Processes of Attitude Change," *Journal of Conflict Resolution,* 2 (1958), 51–60; Robert P. Abelson and Gerald L. Lesser, "The Measurement of Persuasibility in Children," in Hovland and Janis, *Personality and Persuasibility,* Chap. 7; Bert T. King, "Relationships between Susceptibility to Opinion Change and Child-rearing Practices," *op. cit.,* Chap. 10; B. W. Harper and Richard D. Tuddenham, "The Sociometric Composition of the Group as a Determinant of Yielding to a Distorted Norm," *Journal of Psychology,* 58 (1964), 307–311; R. R. Sears, "Dependency Motivation," in M. R. Jones (ed.),

The foregoing qualifications are not meant to deny that some people are more susceptible to persuasive influence than others. They do, however, lead us to question whether all manifestations of yielding to influence should be placed under a single rubric and considered simply as different expressions of the same general predilection. Indeed, conformity may be a behavior category so broad as to be almost equivalent with the concept of learning itself, rather than an identifiable, specific propensity or personality trait that some people possess in significantly greater measure than others.

II. HYPOTHESES AND PROCEDURES

In the present study we are concerned not with conformity in small groups or laboratory settings but with the influences governing the acceptance or rejection of the dominant political and social values of the society. For this purpose we have defined conformity and deviation operationally as the degree to which individual Americans subscribe to the beliefs held by more than 70 per cent of the American people (as measured by survey data). Although this definition differs in certain respects from the definition employed in laboratory studies of conformity (for example, it lacks an explicit reference to possible conflict between a subject's initial beliefs and the forces pressing on him to shift his views), it does contain the two key ingredients in the definition of conformity: 1) subjective concurrence with norms, and 2) objective social pressure—however covert and unsystematic—to adopt the dominant, or majority, view. We consider it plausible to assume that even highly complex, pluralistic societies make a considerable effort to persuade their members to accept prevailing norms. Pressure to conform is implicit in the mere circulation and repetition of majority beliefs, and it is often made explicit in the innumerable group interactions in which those beliefs are directly expressed and reinforced. For convenience we shall refer to persons who embrace (or reject) a disproportionate number of society's beliefs as "conformers" (or "deviants").[20]

In a free and highly diversified society, even moderately informed individuals will encounter and grapple with a bewildering array of beliefs on numerous public questions—all pretending to some measure of legitimacy. Not everyone, however, has equal opportunity or equal capacity to learn which of these beliefs the society approves. The *opportunity* to acquire a society's norms depends on how closely one is stationed to the communications mainstream, and whether one associates with people who reinforce those norms. The *capacity* to absorb and understand what is being communicated depends on education, sensitivity to public affairs, cognitive skills, personality structure, and so on.

Personality structure can affect the learning process—and hence conformity and deviation—in various ways. It can promote or impede social learning *directly,* by enhancing or impairing one's cognitive performance; or *indirectly,* by regulating the nature and extent of one's interactions with others. Given the extraordinary profusion and complexity of beliefs available in modern society, numerous opportunities arise for individuals to select (or avoid) opinions that serve psychological needs. Personality disorders, for example, can determine what an individual hears and what he blocks out; they can distort judgment and cause him to misread signals and messages. Personality traits that affect social adaptation can shape one's interactions with others

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Nebraska Symposium on Motivation, (Lincoln, Nebraska: University of Nebraska Press, 1963), pp. 25-64; McGuire, *op. cit.,* especially pp. 1141-1142.

[19] Lucille Nahemow and Ruth Bennett, "Conformity, Persuasibility and Counternormative Persuasion," *Sociometry,* 30 (1967), 14-25; H. H. Kelly and E. H. Volkart, "The Resistance to Change of Group Anchored Attitudes," *American Sociological Review,* 17 (1952), 453-465.

[20] We are, of course, aware that many who deviate from society's values will conform to the values of their respective subcultures. Correspondence to national norms, thus, is only one of several ways by which one might assess conformity to political beliefs in the society. For some purposes it may not even be the most interesting way. Except for a crude categoric grouping by level of education (see below), we could not, in the present context, analyze the responses of each individual with respect to the several substrata that might be relevant to his political beliefs. We have, nevertheless, investigated conformity and deviation within certain political subcultures, e.g., conformity to party beliefs among the active members of the Democratic and Republican parties, and will report findings in a later publication. There is a depressing paucity of research on conformity and deviation within various political subcultures, a gap which we hope future investigators will soon fill.

so as to impede or promote the learning of norms. Highly defensive, hostile, or paranoid individuals will have more difficulty discerning what others expect them to believe, and will often be unaware that their beliefs diverge from the standard; they will therefore lack incentive for bringing their views into line. Certain personality needs may also lead to a behavior pattern that Crutchfield has called "counterformity"—a tendency to dissent compulsively from the group out of negativistic, hostile, and recalcitrant motives. The counterformist not only resists but is "repelled by the group norms; he seeks to *widen* disagreement between himself and the group . . . to repudiate the group's beliefs or actions even when he perceives that the group is right."[21]

In summary, the tendency to conform to, or deviate from, a society's prevailing attitudes is a function of a complex set of influences, the most significant of which are the following:

1. Social and geographic location, which affect one's opportunity to encounter the beliefs that circulate in the society, and to have them reinforced.
2. Cognitive abilities, which govern one's *capacity* to comprehend those beliefs.
3. Personality characteristics, which influence the clarity, accuracy, and posture with which one perceives and assimilates communications on public questions.
4. Social-psychological factors, which affect social adaptability, including the ability to interact effectively with others, to be aware of the demands of the social environment, etc.

Specifically, then, deviation from majority attitudes should increase with (a) impaired ability or opportunity to encounter and comprehend communications on public questions; (b) personal isolation or estrangement from social institutions and opinion networks; (c) cognitive deficiencies; (d) personality malintegration; and (e) social maladaptation.

Obviously, these hypotheses are fairly broad and could be qualified in several ways. Some people, for example, echo conventional beliefs not because they are informed but because they are intellectually impoverished and respond mindlessly to the opinions they encounter. Others may be receptive to communications on public affairs but, owing to a skeptical turn of mind, question much of what they hear. The college educated, having greater opportunity and capacity to learn the norms than do the less educated, are more likely to respond to the actual content of the beliefs they encounter and, when they deviate at all, are less likely to do so because of personality impediments to effective cognitive performance. Although the hypotheses stress cognitive, personality, and social adaptability factors, other influences (source, content, reference groups, etc.) also affect conformity and deviation. Then, too, conformity does not always stem from the same psychological motives: some individuals may yield out of dependency, others out of personal strength that enables them to accommodate to majority views without threat to their sense of autonomy. These and other cautions suggested earlier are sobering, but their importance for the arguments in this paper can be assessed by the reader after reviewing the findings.

Procedures. The items used to measure conformity were selected from the attitude scales employed in two lengthy questionnaires developed by McClosky for surveys of the general populations of Minnesota and the United States.[22] Both questionnaires contained extensive item pools—539 items in one case and 390 in the other. Some were personality items and others were attitude statements. All were of the agree-disagree type. One questionnaire was administered during the middle 1950's to a cross-section sample of 1082 Minnesota adults, and the other several years later to a national cross-section sample of 1484 adults.

In constructing the conformity measure, we initially selected from each pool every item that had been either endorsed or rejected by at least 70 per cent of respondents from all educational backgrounds—college, high school, and grade school. Our reason for introducing an education hurdle was to insure that the measure would not be education-bound, but would consist of items widely favored (or opposed) by adults in *all* social strata. Approximately 100 items from each item pool met this initial test. These items were then examined and sorted by a panel of 30 judges (drawn from Berkeley staff members and graduate students from the Political Science Department and the Survey Research Center) to select those that clearly reflected political, economic, or social attitudes. The items that survived this sifting were then further screened to reduce subject-matter duplications, to achieve maximum diversity of content, and to eradicate, if possible, any traces of systematic ideological bias.[23] We also tried to correct for possible acquiescent response set by selecting as nearly as practicable an equal number of items on which the majority had agreed and disagreed.

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[21] David Krech, Richard S. Crutchfield, and E. L. Ballachey, *Individual in Society,* New York (1962), p. 507.

[22] Many of the items came from a battery of personality and attitude scales jointly developed by Paul E. Meehl, Kenneth E. Clark, and Herbert McClosky for earlier surveys of political belief, affiliation, and participation.

Two indices for measuring attitude conformity were yielded by these procedures, one from the national survey and the other from the Minnesota survey. The national index consisted of 27 items—16 Agree and 11 Disagree items; the Minnesota index contained 33 items—17 Agree and 16 disagree items. Twenty-one of the 27 items used in the national index also turned up in the Minnesota index. Scalability, of course, was not a criterion in selecting the items for either index.

Sample items from the conformity indices, and the direction of majority preference, follow:

Our freedom depends on the private enterprise system. (Agree)

By belonging to the UN we are running the danger of losing our constitutional right to control our own affairs. (Disagree)

It's the common people who have really made this country great. (Agree)

The idea that everyone has a right to his own opinion is being carried too far these days. (Disagree)

I think it is more important to vote for the man than for the party. (Agree)

There is nothing wrong with a man trying to make as much money as he honestly can. (Agree)

Politicians can't afford to be frank with the voters. (Disagree)

When a community pays a teacher's salary it has the right to tell him exactly what and how to teach. (Disagree)

There is less opportunity in this country than there used to be. (Disagree)

We need a strong central government to handle modern economic problems efficiently. (Agree)

We almost have to restrict the amount of goods we let into this country because labor is so cheap in most other nations. (Agree)

To bring about great changes for the benefit of mankind often requires cruelty and even ruthlessness. (Disagree)

The farmland of this country should be redivided so that no one can own land except the people who actually do the farming. (Disagree)

Poor people should look out for themselves. (Disagree)

No matter what crime a person is accused of, he should never be convicted unless he has been given the right to face and question his accusers. (Agree)

Each respondent was given a conformity score reflecting the sum of his answers to these and the other items in the index. Whenever he answered an item as the majority did (i.e., whenever he was among the 70 per cent or more who agreed, or disagreed, with the item) he was assigned a score of one; whenever he rejected the majority's view, he was assigned a score of zero. The scores on the Minnesota conformity index ranged from 15 to 33, with a mean of 27.7; the scores on the national index ranged from 11 to 27, with a mean of 21.8. To facilitate the analysis, each index was broken into five levels—extreme conformers, moderate conformers, middle, moderate deviants, and extreme deviants—with the cutting points chosen to keep the number of extreme conformers and deviants fairly small and "pure" but large enough to make statistical comparison possible. In the present paper we have simplified the analysis by excluding the respondents who fell into the middle of the distribution; since they neither conformed nor deviated, little was to be gained by including them. (We have, however, run correlations among the variables in which we included the middle group. These correlations merely express, in different form, the same results reported in this paper.) The frequency distributions for the two indices are shown in Table 1.

In the tables that follow we have compared people who largely conform to majority opinion with those who largely deviate from it. The independent variables are the social adjustment, personality, and cognitive characteristics of the

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[23] To this end we decreed that no more than two items could be drawn from the same attitude scale; as it worked out, the items in each of the indices came from at least 20 different scales. It also turned out that the norms selected for use in the final measure were widely accepted by adults not only at different educational levels but also in different age groups and in different size communities. In the non-college sample, conformity to the national norms was higher among younger people than among older, and higher among residents of larger communities than smaller—results that are consonant with the findings reported below. Among those who have attended college, however, the correlations between age, size of community, and conformity with national norms are mixed and inconclusive—which is to say that the response to these values is more homogeneous among the college-educated.

TABLE 1. DISTRIBUTION OF RESPONDENTS ON INDICES OF CONFORMITY-DEVIATION

Level of Conformity	*Minnesota Survey General Population Sample* (N = 1082)			*National Survey General Population Sample* (N = 1484)		
	Range of Scores	Number of Respondents	Percentage of Respondents	Range of Scores	Number of Respondents	Percentage of Respondents
Extreme Deviants	15–21	50	4.7%	11–17	98	6.6%
Moderate Deviants	22–25	179	16.6	18–19	147	9.9
Middle	26–29	511	47.6	20–24	920	62.0
Moderate Conformers	30–31	269	24.1	25–26	279	18.8
Extreme Conformers	32–33	73	7.0	27	40	2.7
		1082	100 %		1484	100 %

respondents, as measured by a number of scales. Most of these scales (Tables 2–6) may be considered "psychological" scales, although not all reflect personality in the same degree. Some of the scales, especially those in Tables 2 and 3, are heavily "social" in content; others, including most (though not all) of the scales in Tables 4 and 5, reflect "personality" characteristics in greater measure. Some of the scales, in short, might be designated as "social-psychological," others as "clinical-psychological." Containing on the average nine items each, the scales were independently developed for inclusion in a battery of psychological tests employed in previous research on political behavior.[24]

In Tables 2–5, mean scores are presented on selected psychological variables for each of the four levels of conformity and deviation. Given our hypotheses and the implicit explanatory model, it might have been more appropriate to turn the tables around and to show conformity in its actual role as the dependent variable. However, since we wanted to compare conformers and deviants as such, we chose to retain these categories intact and to present the comparisons as shown in the tables.

In assessing the findings, the reader should keep in mind that the Minnesota and national surveys, although utilizing many of the same measures, were conducted five years apart on samples drawn from two different universes. They were also separately analyzed. Thus the results derive from two entirely independent studies—one, in effect, replicating the other.

III. FINDINGS

Social Adaptation. The most consistent finding in both surveys is that those who conform to the majority outlook are distinctly better adjusted than those who reject that outlook. With only minor exceptions, the data in Table 2 show that social adjustment variables bear a strong and largely monotonic relationship to conformity-deviation. People who are generally satisfied with their lives, who identify with the society and believe they understand it, who do not feel personally isolated, who do not especially fear the future, and who have a strong sense of social responsibility—tend to accept majority beliefs more often than do those who have adapted less successfully. These findings hold for both the Minnesota and national samples.

While the data in Table 2 are fairly straightforward, their interpretation is more open to conjecture. Social maladaptation is in some measure a function of personality traits that independently influence one's ability to recognize and understand the norms. Hence the connections just observed might in part be a product of common underlying personality factors. There is reason to believe, however, that they are also the direct result of one's capacity for social adaptation as such; as the internal evidence of the present research bears out repeatedly, almost any impediment to social interaction or communication reduces the probability that one will encounter and acquire the values held by most other members of the community.

Although this connection may seem obvious after the fact, it is neither logically nor psychologically inevitable. In the absence of data, one

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[24] The nature and validation of these scales has been set forth in earlier papers. See, for example, Herbert McClosky, "Consensus and Ideology in American Politics," this REVIEW 58 (June 1964); Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus among Party Leaders and Followers," this REVIEW, 60 (June 1960); Herbert McClosky and John H. Schaar, "Psychological Dimensions of Anomy," *American Sociological Review* (February 1965); Harrison Gough, Herbert McClosky, and Paul E. Meehl, "A Personality Scale for Social Responsibility," *Journal of Abnormal and Social Psychology*, 47 (January 1952).

TABLE 2. MEAN SCORES ON MEASURES OF SOCIAL ADAPTATION FOR FOUR LEVELS OF CONFORMITY-DEVIATION FOR MINNESOTA AND NATIONAL GENERAL POPULATION SAMPLES

Level of Conformity	Minnesota Survey (N = 1082)			National Survey (N = 1484)		
	Gen. Pop.	Non-Coll	College	Gen. Pop.	Non-Coll*	College**
Alienation						
Ext. Deviants	4.68 (50)	4.74 (47)	— (2)	4.27 (98)	4.31 (89)	3.35 (8)
Mod. Deviants	3.10 (179)	3.30 (148)	2.16 (31)	3.78 (147)	3.91 (117)	(29)
Mod. Conformers	1.95 (269)	1.94 (175)	1.97 (91)	2.35 (279)	2.42 (187)	2.12 (90)
Ext. Conformers	1.75 (73)	1.82 (39)	1.58 (33)	1.73 (40)	1.85 (27)	(13)
Anomy						
Ext. Deviants	6.72	6.83	—	5.83	6.04	3.43
Mod. Deviants	4.97	5.37	3.06	5.57	6.08	
Mod. Conformers	2.73	3.06	2.00	3.61	3.88	3.14
Ext. Conformers	2.19	2.87	1.39	3.60	3.56	
Bewilderment						
Ext. Deviants	5.06	5.13	—	Scale Not Included		
Mod. Deviants	3.96	4.11	3.23			
Mod. Conformers	3.07	3.18	2.81			
Ext. Conformers	2.93	3.13	2.70			
Community Identification						
Ext. Deviants	3.52	3.51	—	Scale Not Included		
Mod. Deviants	4.10	4.11	4.06			
Mod. Conformers	4.48	4.58	4.27			
Ext. Conformers	4.63	4.72	4.52			
Folksiness						
Ext. Deviants	3.30	3.26	—	Scale Not Included		
Mod. Deviants	3.54	3.61	3.19			
Mod. Conformers	3.77	3.77	3.75			
Ext. Conformers	4.19	4.38	3.94			
Life Satisfaction						
Ext. Deviants	3.54	3.36	—	3.51	3.51	4.30
Mod. Deviants	4.18	3.96	5.23	3.79	3.62	
Mod. Conformers	4.94	4.77	5.25	4.73	4.59	4.97
Ext. Conformers	5.14	5.15	5.18	4.28	4.00	
Pessimism						
Ext. Deviants	3.32	3.36	—	3.36	3.40	2.78
Mod. Deviants	2.79	2.75	3.00	3.28	3.42	
Mod. Conformers	1.92	1.92	1.93	2.27	2.27	2.33
Ext. Conformers	1.68	1.79	1.52	2.48	2.33	
Social Responsibility						
Ext. Deviants	4.02	3.94	—	4.45	4.29	6.32
Mod. Deviants	5.23	4.97	6.48	5.11	4.80	
Mod. Conformers	6.86	6.83	6.96	6.82	6.59	7.26
Ext. Conformers	7.01	6.85	7.30	6.65	6.52	
Status Frustration						
Ext. Deviants	4.32	4.34	—	4.38	4.47	3.00
Mod. Deviants	3.09	3.28	2.19	3.76	3.95	
Mod. Conformers	2.59	2.69	2.40	2.83	2.82	2.74
Ext. Conformers	2.37	2.67	2.00	2.63	2.85	

* Totals for the general population are in a few instances slightly larger than the combined college and non-college totals, because education was not adequately ascertained for a few respondents.

** Owing to the small size of the N's for the Extreme Deviants and Extreme Conformers in the national college sample, we have, in this and subsequent tables, combined the Extreme and Moderate Deviants, and the Extreme and Moderate Conformers, and entered the mean scores only for the combined samples.

might easily predict that the poorly adjusted would be *more,* rather than less, likely to embrace majority outlooks because they fear further separation from the society and therefore seek out opinions that are conventionally anchored. If such influences are at work, they are outweighed by the relative inability of the socially maladjusted to connect with the majority culture and to comprehend its attitudes.

The results on social adaptation hold for both the well-educated and less-educated respondents, but the differences between the conformers and deviants are generally larger and more consistent among those who have not attended college. Among the college educated, such attributes as cognitive capacity, political awareness, and opportunity for communication are more uniformly adequate to the tasks of social learning; hence, deviations from conventional beliefs, while still reflecting maladjustment to some extent, also reflect greater attention to the content of the beliefs to be embraced or rejected.

Cognitive Capacities. We have suggested that agreement with a society's prevailing beliefs may in part be a function of cognitive skills. The assumption here is that intellectual styles and abilities affect both the motivation and capacity to receive and comprehend persuasive communications. If, to borrow McGuire's phrase, cultural norms are, in effect, "obscure, subtle, persuasive messages" which seep in over time,[25] they will in some measure be learned by everyone—but not by everyone *equally.* The intellectually adept will pay closer attention to public affairs and will usually have a better sense of what is valued and likely to be rewarded. They will read the signals more accurately and have a more acute grasp of the intention or meaning of a given public communication.[26] They will detect and respond to persuasive influences that are scarcely perceptible to less alert minds.

Whether these expectations mainly result from raw intelligence, or from cognitive style and disposition, is not clearly known. The experimental findings on the relation of intelligence to conformity and persuasion are inconclusive. Crutchfield and other investigators find meaningful correlations in the conformity experiments, but Hovland and his associates, in their persuasion experiments, do not.[27] McGuire, in analyzing the available research, observes that a positive relation between intelligence and persuasibility is more likely to emerge when reception is complex and comprehension difficult.[28] The weakness of the connection in some situations may be due to the play of conflicting motivations that cancel each other out: superior intelligence may alert an individual to the message, but may also make him more wary of being manipulated and more resistant to arguments that do not satisfy his intellectual standards.

Data relating to the effects of cognitive disposition on our measure of conformity are presented in Table 3. Although no direct tests of intelligence were included in the surveys, several scales were available which measure some aspect of intellectual performance. Their relationship to conformity is generally positive, although stronger in the non-college than in the college samples.

As Table 3 indicates, the conformers are generally more oriented toward intellectual activities than are the deviants, and they attain higher scores on an achievement test of political and social awareness. They also score lower on Acquiescence (this index was mainly designed to test response set, but because it was constructed as a battery of 19 pairs of incompatible items, it also measures the ability to think consistently and discriminatingly).[29] The conformers also have significantly lower scores on the Mysticism scale, which taps the propensity to embrace nonrational, nonscientific, and nonlogical explanations of ordinary phenomena—a propensity that appears from the internal evidence of the study to be most characteristic of people who are intellectually uncritical.

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[25] McGuire, "Personality and Susceptibility," *op. cit.*

[26] Irving L. Janis and Carl I. Hovland, "Postscript: Theoretical Categories for Analyzing Individual Differences," in Hovland and Janis, *op. cit.*, pp. 258–259.

[27] Carl I. Hovland and Irving L. Janis, "Summary and Implications for Future Research," in Hovland and Janis, *op. cit.*, p. 237; Gardner Murphy, *et al., Experimental Social Psychology,* (New York and London: Harper, 1937), p. 930; Carl I. Hovland *et al., Communication and Persuasion,* (New Haven: Yale University Press, 1953), pp. 181–184.

[28] William J. McGuire, "Personality and Susceptibility," *op. cit.*, pp. 1141–1142.

[29] In the Minnesota study, the Agree and Disagree items in the Conformity index are closely balanced out. These results, therefore, cannot be interpreted as an artifact of response set. Furthermore, in the national study, this result is achieved despite the bias in the Conformity index in the opposite direction, i.e. it contains 16 Agree and 11 Disagree items. Hence, other things being equal, the Conformers might be expected to score *higher* than the Deviants on Acquiescence, but, in keeping with our prediction, they score lower.

TABLE 3. MEAN SCORES ON MEASURES OF COGNITIVE DISPOSITION FOR FOUR LEVELS OF CONFORMITY-DEVIATION FOR MINNESOTA AND NATIONAL GENERAL POPULATION SAMPLES

Level of Conformity	Minnesota Survey			National Survey		
	Gen. Pop.	(N = 1082) Non-Coll	College	Gen. Pop.	(N = 1484) Non-Coll	College
			Acquiescence			
Ext. Deviants	7.52 (50)	7.68 (47)	— (2)	6.39 (98)	6.57 (89)	4,10 (8)
Mod. Deviants	5.31 (179)	5.74 (148)	3.23 (31)	6.00 (147)	6.46 (117)	(29)
Mod. Conformers	4.11 (269)	4.46 (175)	3.32 (91)	3.89 (279)	4.14 (187)	3.49 (90)
Ext. Conformers	3.53 (73)	3.97 (39)	2.85 (33)	3.98 (40)	3.81 (27)	(13)
			Political Awareness			
Ext. Deviants	2.78	2.55	—		Scale Not Included	
Mod. Deviants	3.53	2.99	6.10			
Mod. Conformers	5.14	4.49	6.45			
Ext. Conformers	5.44	4.69	6.36			
			Intellectuality			
Ext. Deviants	3.06	2.89	—	3.97	3.81	5.97
Mod. Deviants	3.88	3.51	5.61	4.20	3.76	
Mod. Conformers	5.20	4.83	5.98	5.36	5.00	6.00
Ext. Conformers	5.59	4.85	6.52	5.05	4.86	
			Mysticism			
Ext. Deviants	5.46	5.60	—		Scale Not Included	
Mod. Deviants	4.89	5.20	3.42			
Mod. Conformers	3.37	3.68	2.73			
Ext. Conformers	3.26	3.72	2.70			

The general thrust of these findings is consistent with other data in our surveys which indicate that the deviants, both college and non-college, have failed to absorb the values of the "American Creed"—tolerance, faith in freedom and democracy, political and social equality, procedural rights, etc.—as effectively and thoroughly as have the conformers of comparable education.[30] They are also more prejudiced, more politically cynical, and more responsive to extreme Right-Wing and Left-Wing values—values which the majority, and especially the majority among the educated, strongly reject. That learning capacity should affect these judgments is entirely plausible. Many deviants seem unable to conform even when they want to. In the non-college group, for example, the deviants value conventionality (as measured by our conventionality scale) more than the conformers do. They also profess stronger love of country and greater respect for traditional values. (These findings, however, are less consistent among the college-educated group.) Apparently, the intellectual and psychological failings that initially led some individuals to misread or reject prevailing norms also keep them from discovering how divergent their beliefs actually are. Their inability to utilize the social validation process effectively apparently prevents them from seeing—and hence remedying—their failures to conform.

Note, finally, that the negative correlation between Acquiescence and Conformity adds credence to the claim that conformity (as herein defined) is by no means the same as yea-saying, suggestibility, or gullibility. It suggests, in fact, that the relation is inverse. An individual who affirms belief after belief regardless of content—indeed, regardless of whether one belief contradicts another—is less likely than someone of greater discrimination to recognize which of the innumerable beliefs he encounters have been approved, or rejected, by the society. Being suggestible does not necessarily make him conformist; on the contrary, it makes him vulnerable to many aberrant opinions that a more perspicatious mind would shun.

Self-Esteem. Few personality clusters have received as much attention from students of conformity behavior as self-esteem—the evaluation an individual places upon himself. People *low* in self-esteem are guilt-ridden, cautious, morbidly afraid of failure, chronically anxious, and psychologically vulnerable. People *high* in self-esteem, by contrast, feel a sense of command over themselves and their immediate environment; they are well-integrated, candid, and willing to take risks.

The major assumption about self-esteem and susceptibility to influence is that those who lack self-esteem are highly vulnerable to group pressures. Presumably they will capitulate to pressure either because they fear ridicule if they should prove to be "wrong," or because they yearn for group approval and a sense of interpersonal solidarity. Lacking self-confidence, they are in any event inclined to doubt the worth of their own judgments. For these and other reasons, individuals low in self-esteem have tended under experimental conditions to be uncommonly responsive to group influence. However, the relationship between self-esteem and yielding is not always monotonic, for the anxiety that often attends low self-esteem can function either to inhibit or potentiate persuasibility, depending upon the magnitude of the anxiety aroused by the stimulus.[31]

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[30] Since some of the items used in the Conformity index were taken from the scales measuring attitudes toward these values, the relation between conformity-deviation and these attitudes scales partly reflects overlapping item content. However, the differences between conformers and deviants on these and other attitude measures cited in the paragraph are larger than could be accounted for by item overlap alone.

We predicted that anxiety, and low ego-strength in general, would serve to impede the learning of community norms. The very traits that might lead people with low self-esteem to yield in a laboratory would, in the maelstrom of the complex larger society, insulate them against influence. Lacking self-assurance, such people tend to withdraw from social interactions and avoid "involvement" in group activities.[32] McGuire suggests that they are frequently preoccupied with themselves and too distracted to attend closely to outside communications.[33]

In Table 4 we present data on scales that tap various facets of self-esteem. Need Inviolacy, for example, measures vulnerability to psychological exposure and the fear of being unmasked; the other scales reflect one's sense of personal security and command (Dominance and Self-Confidence), propensity to heightened feelings of shame and regret (Guilt), and a proclivity toward excessive worry, restlessness, inability to concentrate, and pervasive feelings of uncertainty (Manifest Anxiety and Personality Disorganization).

Among the non-college populations in both the national and Minnesota studies, the deviants consistently show up as having less self-esteem than do the conformers. Among the college educated, however, the prediction is supported in only four of the nine instances in which the connection is tested (Dominance, Guilt, Anxiety, and Need Inviolacy—all in the Minnesota sample). No differences turn up in three instances and reversals appear in the other two. We can only guess at the exact meaning of these instances of disconfirmation, but the evidence from our own and other research suggests that the better educated have sufficient ability and opportunity to learn the community norms despite personality needs that are in other ways disabling. Then, too, they are likely, as we have observed, to weigh the content of beliefs more carefully—which in the case of a content-diversified index would tend to flatten differences further.

Rigidity and Aggression. The relation of personality rigidity and aggression to conformity and persuasibility has also been explored experimentally, but with equivocal results. In general, experiments on conformity have usually hypothesized positive correlations with rigidity, hostility, and authoritarianism; while experiments on communication and persuasion have sometimes predicted positive and sometimes negative correlations. One can reason either way: that hostile and rigid subjects should be more authoritarian and hence ready to submit to conformity pressures; or, alternatively, that they should prove especially recalcitrant in resisting efforts to persuade them to shift their point of view.

Unfortunately the research findings from the experimental literature are not sufficiently consistent to resolve these issues. Crutchfield[34] and others have found positive correlations between conformity and such personality dispositions as hostility and authoritarianism. Linton and Graham[35] have found persuasibility to be related to the aggressive and submissive aspects of authoritarianism but not to authoritarianism as a

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[31] See, for example, William J. McGuire, "Personality and Susceptibility to Social Influence," *op. cit.*, pp. 1142–1143. For a recent review of the relation between anxiety and learning generally, see Janet Taylor Spence and Kenneth W. Spence, "The Motivational Components of Manifest Anxiety: Drive and Drive Stimuli," in Charles D. Spielberger (ed.), *Anxiety and Behavior,* (New York: Academic Press, 1966), pp. 21–26; and Charles D. Spielberger, "The Effects of Anxiety on Complex Learning and Academic Achievement," in Spielberger, *op. cit.*; S. Sarason, K. Davidson, F. Lighthall, R. Waite, and B. Ruebush, *Anxiety in Elementary School Children,* (New York: Wiley, 1960).

[32] See, for example, Morris Rosenberg, *Society and the Adolescent Self-Image,* (Princeton: Princeton University Press, 1965); Stanley Coopersmith, "Studies in Self-Esteem," *Scientific American* (February 1968), 96–106, and *The Antecedents of Self-Esteem,* (London: W. H. Freeman & Co., 1967).

[33] McGuire, "Personality and Susceptibility to Influence," *loc. cit.*

[34] Crutchfield, "Conformity and Character," *loc. cit.*

[35] Harriet Linton and Elaine Graham, "Personality Correlates of Persuasibility," in Hovland and Janis, *Personality and Persuasibility,* pp. 69–101.

TABLE 4. MEAN SCORES ON PERSONALITY MEASURES OF SELF-ESTEEM FOR FOUR LEVELS OF CONFORMITY DEVIATION FOR MINNESOTA AND NATIONAL GENERAL POPULATION SAMPLES

Level of Conformity	Minnesota Survey (N = 1082)			National Survey (N = 1484)		
	Gen. Pop.	Non-Coll	College	Gen. Pop.	Non-Coll	College
Dominance						
Ext. Deviants	2.26 (50)	2.15 (47)	— (2)	3.27 (98)	3.21 (89)	512 (8)
Mod. Deviants	3.35 (179)	3.08 (148)	4.65 (31)	3.58 (147)	3.11 (117)	(29)
Mod. Conformers	4.43 (269)	3.77 (175)	5.76 (91)	4.06 (279)	3.71 (187)	4.69 (90)
Ext. Conformers	4.85 (73)	4.13 (39)	5.70 (33)	4.23 (40)	4.26 (27)	(13)
Guilt						
Ext. Deviants	5.12	5.23	—	4.92	5.00	3.62
Mod. Deviants	4.70	4.82	4.10	4.76	5.06	
Mod. Conformers	3.87	4.10	3.40	3.97	3.97	4.05
Ext. Conformers	3.75	4.03	3.33	4.05	3.89	
Manifest Anxiety						
Ext. Deviants	4.50	4.47	—	Scale Not Included		
Mod. Deviants	3.96	4.05	3.48			
Mod. Conformers	3.20	3.28	3.01			
Ext. Conformers	3.10	3.18	2.94			
Need Inviolacy (Ego Vulnerability and Defense)						
Ext. Deviants	4.04	4.13	—	3.45	3.55	2.38
Mod. Deviants	3.20	3.36	2.39	3.54	3.81	
Mod. Conformers	2.54	2.67	2.23	2.57	2.63	2.35
Ext. Conformers	2.11	2.10	2.12	2.10	2.33	
Personality Disorganization						
Ext. Deviants	4.04	4.11	—	Scale Not Included		
Mod. Deviants	3.02	3.18	2.26			
Mod. Conformers	2.30	2.45	2.00			
Ext. Conformers	2.29	2.31	2.24			
Self-Confidence						
Ext. Deviants	2.94	2.89	—	Scale Not Included		
Mod. Deviants	3.26	3.04	4.32			
Mod. Conformers	3.99	3.63	4.69			
Ext. Conformers	3.62	3.13	4.21			

whole. Other investigators[36] contend that authoritarians yield in conformity experiments principally when the message is so simple and easily understood that individual differences in capacity for attention and comprehension are negligible. Still others[37] find that persuasibility among aggressive individuals may depend on whether the communication is benign or punitive. Janis and Hovland believe that "personal rigidity" may chronically lead subjects to refuse to accept persuasive messages.[38] In short, the existing research bears out the complexities that surround the interplay of these variables and underscores our earlier observation that the connection between conformity and personality structure is rarely a simple linear relation that can be predicted apart from the context in which they coexist.

In light of our own earlier research, we expected to find rigidity and hostility correlating negatively with the adoption of majority values —largely because these personality traits tend to retard social learning and hinder the social interactions through which beliefs are disseminated.[39] If the members of a society acquire its norms through gradual assimilation—through the repeated exchange of ideas, the clarification of subtle distinctions, and the modification of opinions resulting from the interplay of divergent views—one can plausibly assume that people gripped by hostile and inflexible needs will engage less often in these activities and will be less likely to learn what the society really believes. Unusually hostile or inflexible people, moreover, tend to be insensitive to the qualifications, contingencies, and nuances that are inherent in many social norms—an incapacity that can easily lead to misunderstandings or misapplications of those norms.

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[36] See, for example, Homer H. Johnson *et al.*, "Effects of Source Credibility," *op. cit.*

[37] Walter Wise and B. J. Fine, "The Effect of Induced Aggressiveness on Opinion Change," *Journal of Abnormal and Social Psychology*, 52 (1956), 109–114.

[38] Janis and Hovland, "Postscript," *op. cit.*, p. 257.

[39] Herbert McClosky, *Political Inquiry*, (New York: Macmillan, 1969), pp. 74 and 87.

The data in Table 5 generally confirm these expectations. In all but two instances (the scores on Authoritarianism and Rigidity for the national college-educated sample) the deviants are more hostile and inflexible than the conformers. They are more paranoid (a projected expression of hostility), more intolerant of people whose traits or opinions they disapprove of, more distressed by ambiguity, more punitive, and more jaundiced in their estimates of other people. Contrary to the claims in the popular literature, those who conform in matters of political belief (as distinguished from those who merely glorify conformity) are less motivated than the deviants by a desire to force everyone to obey prescribed standards.

IV. SUMMARY AND DISCUSSION

The findings from the two surveys reported in this paper confirm the hypothesis that conformity-deviation—defined operationally as agreement or disagreement with prevailing American beliefs on diverse social and political subjects—is significantly related to personality factors. Those who preponderantly conform to majority attitudes are by substantial margins better adapted socially and psychologically than those who preponderantly reject them. They are more

TABLE 5. MEAN SCORES ON PERSONALITY MEASURES OF AGGRESSION AND RIGIDITY FOR FOUR LEVELS OF CONFORMITY-DEVIATION FOR MINNESOTA AND NATIONAL GENERAL POPULATION SAMPLES

Level of Conformity	Minnesota Survey			National Survey		
	Gen. Pop.	(N = 1082) Non-Coll	College	Gen. Pop.	(N = 1484) Non-Coll	College
			F Authoritarianism			
Ext. Deviants	5.12 (50)	5.23 (47)	— (2)	6.41 (98)	6.67 (89)	3.62 (8)
Mod. Deviants	4.70 (179)	4.82 (148)	4.10 (31)	6.33 (167)	6.72 (117)	(29)
Mod. Conformers	3.87 (269)	4.10 (175)	3.40 (91)	5.23 (279)	5.52 (187)	4.05 (90)
Ext. Conformers	3.75 (73)	4.03 (39)	3.33 (33)	5.23 (40)	5.19 (27)	(13)
			Faith in People			
Ext. Deviants	2.04	1.98	—	Scale Not Included		
Mod. Deviants	2.71	2.83	2.13			
Mod. Conformers	3.86	4.01	3.56			
Ext. Conformers	4.37	4.15	4.64			
			Hostility			
Ext. Deviants	6.40	6.55	—	4.14	4.33	3.05
Mod. Deviants	4.74	4.99	3.58	3.93	4.09	
Mod. Conformers	2.89	3.05	2.53	3.08	3.14	2.66
Ext. Conformers	2.53	2.90	2.00	3.10	3.19	
			Intolerance of Ambiguity			
Ext. Deviants	6.00	6.26	—	5.76	5.96	4.11
Mod. Deviants	5.64	5.85	4.61	5.64	5.96	
Mod. Conformers	4.40	4.73	3.69	4.31	4.55	3.85
Ext. Conformers	3.82	3.90	3.73	4.23	4.26	
			Paranoid Tendencies			
Ext. Deviants	6.40	6.55	—	5.37	5.54	3.89
Mod. Deviants	4.74	4.99	3.58	5.31	5.60	
Mod. Conformers	2.89	3.05	2.53	3.04	3.25	2.66
Ext. Conformers	2.53	2.90	2.00	2.88	2.81	
			Rigidity			
Ext. Deviants	3.74	3.83	—	3.62	3.75	2.35
Mod. Deviants	3.21	3.36	2.48	3.28	3.47	
Mod. Conformers	3.05	3.12	2.89	2.99	3.01	2.94
Ext. Conformers	2.56	2.95	2.06	2.72	2.67	
			Tolerance			
Ext. Deviants	4.60	4.51	—	4.77	4.60	6.24
Mod. Deviants	5.30	5.22	5.68	5.39	5.21	
Mod. Conformers	6.41	6.33	6.57	6.61	6.50	6.85
Ext. Conformers	6.66	6.46	6.91	7.10	7.15	

intellectually oriented, more politically aware, and more proficient in their cognitive skills. They exhibit greater self-esteem and less anxiety than the deviants, and are less motivated by aggression and inflexibility.

The evidence is consistent with the hypothesis that the conformers are both more able and more likely to encounter and comprehend the norms—contingencies which the experimental research shows to be of central importance in affecting communication and persuasion. They are more accessible to interaction, less withdrawn or socially isolated, and more strongly motivated to receive and respond to information on public questions. Although we have not tested the communication process directly, we think it plausible to infer from the evidence that the deviants, more often than the conformers, have missed or misread communications on public norms—an inference consistent with the findings from our previous research on the correlates of anomy, isolationism, extreme Right-Wing and Left-Wing radicalism, and other outlooks which diverge from prevailing American attitudes.[40]

Despite the pronounced tendency of the findings, some puzzling questions remain. One question, for example, concerns the meaning of the deviant responses: Are they genuine expressions of deviant belief or should they be regarded instead as "non-attitudes"—i.e., labile or casual responses by people so little interested in the substance of the items that they score as deviant largely by answering erratically or randomly?[41] Might there even be reason to believe that many of the deviants would show up as conformers if tested on another occasion?

There is little doubt that some people respond to questionnaire items—as they do to the public opinions they encounter in their daily activities—without much thought. Indeed, it is an argument of this paper that many people hold nonconforming attitudes not because they are led by superior sensitivity to weigh alternatives and to reject conventional views, but mainly because they have been so imperfectly socialized that they are either indifferent to many social issues or unable to recognize what the society believes. This is not to say, however, that their responses to attitude items are entirely random or casual, for one could not then explain the distinct pattern of correlations yielded by the research reported here—a pattern predictable from theory, consistent with our earlier research on other deviant attitudes, and reproduced here in two independent studies conducted several years apart on samples from two different universes.[42]

Even if we assumed that some respondents *had* attained deviant scores on the Conformity index by answering randomly or thoughtlessly, they could not, by replying in this fashion, have achieved the highly patterned, and often extreme, scores on the numerous personality and attitude scales with which the conformity index is correlated. Since the items from the various measures were scrambled and presented to the respondents in a single, unstructured item pool, it would be highly implausible to suppose that they responded randomly or heedlessly to the items that were later selected for the Conformity index while replying carefully and "accurately" to the numerous other items with which the Conformity items were intermingled. The assumption that indifferent respondents might be accounting for the results becomes still less tenable when one considers that such respondents would turn up not only among the deviants but also among the conformers, if only because they can be expected, *ceteris paribus,* to encounter majority views more often than they encounter minority views—when they encounter them at all. Note, finally, that no measure employed here has consisted merely of a single item or question; all have been multiple item batteries or scales, averaging close to nine items each for the independent variables and between 27 and 33 items for the dependent Conformity indices. This feature, of course, greatly reduces the possibility that scores at either extreme could be achieved randomly or without reference to actual attitude content.[43]

Let us assume for the sake of the argument, however, that some of the deviants are in fact expressing non-attitudes or responding randomly. Are the results markedly different for those respondents who are more thoughtful, more discriminating, more informed, and more concerned with public questions?

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[40] McClosky, *ibid.,* Chaps. 2 and 3.

[41] Cf. Philip E. Converse, "Attitudes vs. Non-Attitudes: Continuation of a Dialogue," paper read at Seventh International Congress of Psychology, Washington, D.C., 1963, (mimeo).

[42] Similar correlation patterns have also emerged from our study of deviation and conformity among Democratic and Republican party leaders toward the dominant beliefs of their respective parties. For a preliminary report of these findings, see "The Influence of Personality on Political and Social Attitudes," *Mental Health Program Reports*—3, National Institute of Mental Health, Chevy Chase, Maryland, January, 1968, pp. 105–106.

[43] Converse, *loc. cit.*

TABLE 6. CONFORMERS AND DEVIANTS COMPARED, CONTROLLED FOR MIDDLE ACQUIESCENCE AND HIGH AWARENESS—MINNESOTA SURVEY ONLY
(Percentages Down)

Scales	Total Minnesota Sample		Middle Acquiescence Sample		High Awareness Sample	
	Deviants N = 229	Conformers N = 342	Deviants N = 58	Conformers N = 128	Deviants N = 54	Conformers N = 160
Alienation						
% High	40.2	16.4*	34.5	18.0*	25.9	14.4*
% Low	21.4	47.7	19.0	48.4	25.9	52.5
Anomy						
% High	56.8	10.8*	53.4	8.6*	25.9	3.7*
% Low	19.7	53.2	13.8	54.7	46.3	63.1
Bewilderment						
% High	48.0	21.6*	36.2	18.7**	14.8	16.4
% Low	19.2	41.5	15.5	39.1	42.6	46.2
Community Identification						
% High	14.8	24.0***	15.5	28.9	14.8	20.6
% Low	35.8	20.8	36.2	22.7	37.0	21.9
Folksiness						
% High	21.4	32.2**	24.1	36.7	14.8	34.4**
% Low	49.3	35.4	39.7	32.0	59.3	35.0
Life Satisfaction						
% High	24.0	42.1*	22.4	46.9**	40.7	47.5
% Low	40.2	24.9	39.7	24.2	24.1	20.0
Pessimism						
% High	60.7	26.9*	58.6	30.5**	53.7	26.2*
% Low	17.0	42.1	22.4	36.7	22.2	41.2
Social Responsibility						
% High	10.5	36.3*	17.2	33.6*	20.4	40.6**
% Low	59.3	18.4	56.9	20.3	31.5	15.6
Status Frustration						
% High	44.1	27.8*	32.8	29.7	35.2	19.4
% Low	18.3	29.2	24.1	28.9	29.6	36.9
Acquiescence						
% High	51.1	21.9*	—	—	22.2	10.6**
% Low	23.6	40.6	—	—	59.3	47.5
Political Awareness						
% High	15.3	34.2*	8.6	35.9	—	—
% Low	61.1	26.6	60.3	21.1	—	—
Intellectuality						
% High	15.7	48.5*	15.5	46.9*	35.2	58.7*
% Low	52.8	17.8	50.0	14.8	27.8	10.0
Mysticism						
% High	59.8	24.6*	46.6	21.9**	31.5	16.9**
% Low	11.4	32.5	15.5	34.4	25.9	46.9

Table 6 (*Cont.*)

Scales	Total Minnesota Sample		Middle Acquiescence Sample		High Awareness Sample	
	Deviants N = 229	Conformers N = 342	Deviants N = 58	Conformers N = 128	Deviants N = 54	Conformers N = 160
Dominance						
% High	21.0	51.2*	19.0	53.9*	42.6	63.7*
% Low	42.8	20.8	41.4	14.1	11.1	13.1
Guilt						
% High	39.7	20.8*	32.8	16.4**	25.9	14.4
% Low	24.9	47.7	25.9	51.6	40.7	53.1
Anxiety						
% High	33.2	14.9	25.9	14.8	24.1	11.2
% Low	20.5	31.3	24.1	35.2	27.8	32.5
Need Inviolacy						
% High	49.3	24.3*	36.2	20.3**	31.5	19.4
% Low	27.5	54.4	39.7	63.3	48.1	61.2
Personality Disorganization						
% High	43.2	24.3*	36.2	23.4	29.6	18.7
% Low	21.8	36.5	34.5	39.1	31.5	35.6
Self-confidence						
% High	22.7	37.1*	20.7	38.3***	29.6	50.0***
% Low	35.4	22.5	41.4	22.7	25.9	14.4
F. Authoritarianism						
% High	44.5	14.3*	39.7	10.9*	9.3	5.6
% Low	17.9	38.0	8.6	36.7	46.3	46.9
Faith in People						
% High	10.5	40.1*	10.3	35.9*	18.5	40.6*
% Low	52.0	20.5	48.3	22.7	53.7	23.1
Hostility						
% High	60.7	33.0*	56.9	37.5***	33.3	27.5
% Low	19.7	42.7	22.4	42.2	42.6	50.0
Intolerance for Ambiguity						
% High	60.7	27.8*	58.6	23.4*	37.0	18.1**
% Low	16.2	36.5	10.3	35.9	46.3	45.6
Paranoia						
% High	59.8	19.6*	53.4	17.2*	31.5	13.7**
% Low	13.1	46.8	12.1	40.6	29.6	51.9
Rigidity						
% High	49.8	35.7**	36.2	39.1	25.9	26.2
% Low	26.6	33.9	29.3	32.8	46.3	41.9
Tolerance						
% High	22.7	51.5*	27.6	57.8*	31.5	56.9*
% Low	60.3	20.2	55.2	19.5	51.9	17.5

% Middle has been omitted to conserve space.
* Chi Square for table significant at or below .001 level.
** Chi Square for table significant at or below .01 level.
*** Chi Square for table significant at or below .05 level.

We have already seen (Tables 2–5) that while the differences between well-educated conformers and deviants are usually smaller than they are for the less-educated, the pattern of differences is generally similar for both samples. In Table 6 we have put the issue to two other, more severe, tests. There we present data on two narrowly restricted subsets of respondents—those who, on an achievement test, display high levels of political awareness and understanding; and those who score close to the median on the Acquiescence index,[44] and who therefore appear to have answered the items with greater care and discrimination. Neither group is likely to have responded randomly or thoughtlessly.

Both Awareness and Acquiescence are measures of cognitive capacity and, as we have seen, are correlated with conformity—one positively, the other negatively. Neither, however, is an exhaustive measure of this capacity, for not only are they imperfect as measures, but other factors, such as personality structure, can and often do have an independent, direct effect on the clarity and understanding with which norms are learned. Thus, respondents with similar scores on Awareness (or Acquiescence) may still differ somewhat in their ability to read communications correctly and discern their implications. Controlling for these measures, we therefore assumed, should reduce the magnitude of the differences between conformers and deviants but not eradicate them entirely. This assumption proved, on the whole, correct.

The results confirm that the psychological differences between conformers and deviants reported for the total sample (and shown in Table 6 in percentage form) hold in most instances even for the more thoughtful and politically informed respondents—although they are, as predicted, smaller for the more narrowly defined subsamples. The differences that emerge are in the predicted direction, and are consistent with the general findings in all but a few instances. Moreover, in 20 of the 25 comparisons with middle Acquiescence controlled, and in 16 of the 25 comparisons with high Awareness controlled, the differences remain large enough to be significant, by chi square test, at or well below the .05 level of significance—despite the severity of the controls and the reduction in sample size. Similar tables, controlling for Acquiescence, were also computed for the *national* sample, with results at least as strong as those reported here, and in some cases (e.g., rigidity and status frustration) markedly stronger.

Divergence from majority norms among the more sophisticated respondents, of course, can scarcely be explained by reference to distance from communication networks, lack of information, or insufficient opportunity to encounter the norms. For some, it doubtless represents a considered, philosophically anchored rejection of the established society and its values. For others, however, it appears to be strongly associated with cognitive and personality impairments that hinder effective communication and the accurate learning of norms, or that lead, in some cases, to a "counterformist" rejection of prevailing standards.

Since the data from the two surveys reported in this paper were gathered in the middle and late '50's, one may ask whether they would hold with equal force today. Defiance of traditional norms and accepted standards appears to be more common in the '60's than in the decades that preceded it. In certain quarters today, nay-saying is "in," and may itself have become the norm. Today's deviants then, may conceivably differ in crucial ways from those described in this paper.[45] The question, of course, cannot be settled without a new study of comparable design on comparable samples. There are, nevertheless, reasons to expect that the results of such a study would be similar to those reported here. For one thing, historical studies of values indicate that a society's basic beliefs are extraordinarily tenacious and change extremely slowly. For another, even if the norms *had* changed dramatically in the space of a decade, the social-psychological processes by which people become aware of and acquire norms can scarcely have changed much. No matter what beliefs prevail in a society, some people will be better placed, better adapted, better trained, and better disposed than others to discern and appreciate them.

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[44] We have chosen the Minnesota, rather than the national survey for this test, partly because it contains all the scales considered in the earlier tables; partly because it includes the Awareness scale, which was omitted from the national survey; and partly because the numbers of Agree and Disagree items in its Conformity index were more closely balanced.

[45] For a recent study of one such deviant subculture, see David Whittaker and William A. Watts, "Personality Characteristics of a Nonconformist Youth Subculture: A Study of the Berkeley Non-Student," *Journal of Social Issues*, 25 (1969), 65–89. The members of this special, rather exotic, but by no means homogeneous community resemble our deviants in certain respects (e.g. they are, compared with enrolled students, more "socio-emotionally maladjusted," alienated, anomic, personally less integrated, express lower self-esteem and greater anxiety, etc.); but they differ in important other respects (e.g. they are more autonomous, freer in their impulse expression, and have a more experimental, intellectual, and complex orientation toward social experience.)

A final question concerns the apparent conflict between the findings reported in this paper and those turned up by Crutchfield in his personality assessment of subjects who conform in an Asch-type laboratory experiment. A comparison of the personality profiles of conformers and deviants in the two studies reveals that the *deviants* in our study closely resemble the *conformers* in the Crutchfield experiment. Despite the seeming inconsistency, we believe that the findings of the present study are not only congruent with Crutchfield's but in a curious way confirm them. Both Crutchfield's conformers and our deviants suffer cognitive and personality deficiencies that lead to distorted perceptions, heightened anxiety, narrowness and inflexibility of focus, defective capacity for observing situations accurately, insensitivity to signals that indicate how one is expected to respond, and defective antennae that prevent them from sensing what is happening and how they are supposed to react. That such people conform in the one study and deviate in the other largely reflects the differences in the two situations and research tasks.[46] In the conformity experiment, the stimulus and the research task are simple, the source is identifiable, the pressure is tangible, the direction of the influence is unmistakable; and the means of satisfying the demands and relieving one's internal conflicts are or appear to be immediately available—namely, by conforming to the majority judgment. In the society at large, however, one encounters innumerable, varied, and often conflicting messages, many of them issuing from unknown sources and most of them difficult to comprehend; the pressure to conform is frequently diffuse, intangible, and indirect; the way to resolve conflicting pressures is often unclear or unavailable; the reinforcements for choosing the "correct" alternative are uncertain and sporadic—in short, one is beleaguered by the sheer number and variety of influences that compete for recognition. Under these conditions, those least able to master the environment are most likely to become the "deviants." In the Asch-Crutchfield experiments they are most likely to become the "conformers."

All this points up the risks in assuming that all forms of conformity (or deviation) are phenotypically or genotypically equivalent. Much depends on the source, the context, the reference group, the content, and the like. Even the several forms of susceptibility to influence are only weakly, and sometimes negatively, correlated. In speaking of conformity and deviation, one needs to take into account much more than the simple question of whether an individual yields to or resists influence. When one observes this behavior, he has merely begun the inquiry.

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[46] Support for this interpretation may be inferred from a newly published study by Laurence J. Gould, "Conformity and Marginality: Two Faces of Alienation," *Journal of Social Issues,* 25 (1969), 39–63. Gould finds that the highly alienated (who closely resemble our deviants, e.g., more acquiescent, more suspicious, more expressed pathology, less self-esteem, and, of course, more alienated) also conform more in an Asch-type experiment. They also exhibit less curiosity about the reasons for their own judgments and those of the majority, "are less sensitive to the feelings of others [and] are less likely to be 'tuned in' to the more subtle and informal norms in any given social situation." See also Melvin Seeman, "An Experimental Study of Alienation and Social Learning," *American Journal of Sociology,* 49 (1963), 270–284.

POLITICAL MONEY

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I. MONEY AND POWER[1]

Money, Primitive and Modern

The basic institutions of modern democracy were established at a very early stage in the transformation that societies have undergone since feudalism. In their establishment, forms that had developed even before feudalism, in Greece and Rome, were copied. Thus the institutions are very old ones indeed.

When the age of these political institutions is compared to that of economic institutions, the contrast is sharp and striking. As society has become more and more rationalized, economic transactions have mirrored this ever-increasing rationality, with increasing technical sophistication, and increasing abstraction. The best indicator of this is in the role of money. From barter economies to modern economies in which bank-deposit money and credit account for most transactions, the development of economic mechanisms for effecting exchange has been very great. Yet the development of political institutions, and of mechanisms for effective political authority, has been far less great.

In this paper, I want to explore the similarities and differences between political power and the embodiment of economic power or value, that is, money. A careful examination of the differences will suggest which of the differences are intrinsic to the differing natures of economic and political transactions, and which are accidental. This will then allow raising questions about what kinds of innovations in political institutions might be feasible, and might allow these institutions to develop more compatibility with the technical and economic changes that occur with such rapidity in modern society. The development of modern money and monetary institutions acted to greatly facilitate manufacture, and thus to satisfy wants through the economic system. It is conceivable that analogous developments in political institutions could provide similar benefits.

A review of relevant points in the development of modern money.

One of the first steps away from the primitive economic system of barter is the use of commodity money: some valued commodity which could serve as a medium of exchange, a store of value, and a unit of account. The use of gold, silver, and other precious-metal coins which were passed by weight was only a sophisticated version of this, for the value of such coins was always regulated by their commodity value for non-monetary uses.

However, a major innovation in economic institutions occurred when notes, or promises to pay, themselves came to be transferred from hand to hand, and used no longer as a private debt, but as a transferable obligation. The importance of this new development was extremely great, because it was a qualitative change in economic institutions. It meant that an obligation that existed on the part of A toward B could be used by B in a relation with C as a valuable commodity, in paying off an obligation he had toward C.[2]

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[1] My attention was drawn to the comparison of money and power by a paper of Talcott Parsons, "On the Concept of Influence," *Public Opinion Quarterly,* 27 (Spring 1963), 37–62. See also my detailed critique of this paper, and Parsons' reply, James S. Coleman, "Comment on 'On the Concept of Influence'", *Public Opinion Quarterly,* 27 (Spring 1963), 63–82, and Parsons, *ibid.,* pp. 87–92. I am greatly indebted to Parsons for this initial stimulation.

[2] This is not to say that chronologically this development occurred late in the history of economic institutions, nor that in every society it followed the development of commodity money. In barter systems, the absence of a monetary unit has often led to a system of widespread obligations or debts, these obligations serving in place of a system of money, to allow nonsimultaneous exchanges to take place. There seem to be no good examples in primitive monetary systems of such obligations being systematically transferred and used as legal tender. (See, for example, Paul Einzig, *Primitive Money,* 2nd edition, London, 1966, pp. 365–366.) However, Hawtrey describes the possibility of such a system, where in a barter economy, a medium of exchange is made unnecessary by a balancing off of debts and credits by the parties in the market. (See R. G. Hawtrey, *Currency and Credit,* 3rd edition, London, 1928.) While this is not so extensive a system of transfer of the obligation from a creditor to a third party as the institution of legal tender, it does represent a transfer of the obligation to a third party, if only for balancing accounts.

It is likely that in a small, closely-knit community, a direct transfer of obligations could operate to the extent of cancelling out obligations in short cycles. That is, if A owed B, and B owed C, and C owed A, and all three obligations were of the same amount, then clearly there would be little hesitation on the part of these three to transfer obligations and settle accounts. However, in an extended chain, if A owes B owes C owes D owes E owes F owes A, then suppose B attempted to use A's obligation to pay off his own, and so on down the line; the transaction might be cut short at D, who perhaps does not know or does not trust A, and would thus not accept A's note in place of that of his direct debtor. Thus it is easy to see that such a system would be impeded whenever the circle of exchange went beyond a group who enjoyed equal degrees of one another's confidence. In short, such chains of obligation, with the note of the head of the chain serving as currency, do not constitute a viable form of money.[3]

It is for this reason that the notes, or promises to pay, came to be notes of a single party. For the party which issues notes as promises to pay, that is, a bank, must enjoy a degree of confidence or trust greater than that of almost any other person in the community. For if a bank note is not at least as acceptable to a creditor as is the debtor's note itself, then the creditor would reject it, and the bank notes would fail to circulate, but would stop, and accumulate in the hands of the more trustworthy men, whose own credit notes or promises to pay would circulate. The failure to circulate would itself quickly lead to a repercussion along the line, and a backwave of refusal to accept the notes, leading directly back to the bank's issue.

The development of such a system of obligations as a replacement for commodity money is exemplified in 18th Century England, during the Industrial Revolution. Outside London, a merchant, such as a grain dealer, would begin to specialize by issuing notes or promises to pay, coming to take on the role of banker for the community. These notes, though issued as an obligation of the issuing party, the merchant, were intended to be used as currency, and came to be accepted as such. The distance from London, and greater confidence in local merchants, made these notes or obligations more acceptable locally than those of the Bank of England, located in London, and the merchant's reputation made them more acceptable than the note of a creditor himself. Only later, toward the middle of the 19th Century, with the geographic widening of trade, the need for a commonly accepted currency, and the failures of some country note-issuers, did the obligations of the Bank of England, in the form of its notes, come to be more widely accepted throughout England than the country merchant-bank notes.

It is clear that the use of these debts or obligations greatly facilitated manufacture, because of the inflexibility of supply of hard currency, that is, coin, and the increasing volume of economic transactions that required payment in money. Thus the development of monetary institutions based on trust was an important aid to the increased satisfaction of wants.

In advanced economies today, of course, nearly all money consists of obligations or debts which are exchanged, excluding only some coin which has a commodity value near its face value. Currency is a direct obligation of the government whose treasury issues it; a bank deposit (which constitutes the other major form of money) is an obligation of the bank to pay in currency which itself is an obligation of the government. (In the United States, the lack of confidence in banks after 1932 led to the Federal Deposit Insurance Corporation, which in effect transfers the bank's obligation to that of the Federal Government.) In nearly all transactions between two parties other than a bank or a government, however, the obligation or promise that changes hands is an obligation of a *third* party, that is the government (in the case of treasury notes) or a bank (in the case of a check drawn on a bank). The obligations of individual citizens, such as the credit accounts of a merchant's customers or the credit accounts of credit card companies or finance companies are obligations less readily accepted as money, ordinarily only as illiquid assets, which can be transferred only at a discount. Detailed knowledge of the use and transferability of such debts would be necessary in order to determine how close to money they become. In any case, it is clear that the general drift of industrial societies has been toward acceptance of an ever-widening range of obligations as money, presumably with the growth of confidence in the indebted institutions. It is probably true that today the debt of an ordinary citizen is not discounted more when accepted by a bank than was the note of an employer in 1880 in Lancashire when accepted by merchants.[4]

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[3] There is one important historical example in which such chains of obligation did constitute a form of money. This was in the early industrial activity of Lancashire in England. (For an account of the use of bills of exchange in Lancashire, see T. S. Ashton, "The Bill of Exchange and Private Banks in Lancashire, 1790–1830," *Economic History Review,* Vol. 15, Nos. 1, 2, 1945, reprinted in *Papers in English Monetary History,* edited by T. S. Ashton and R. S. Sayers, Oxford University Press, London, 1953.) While in most areas of England in the late 18th century, the notes of merchant bankers or goldsmiths became accepted currency, in the Lancashire industrial area, these bank notes were less freely accepted (and then only with a discount) than bills of exchange. Bills of exchange were debts incurred by one manufacturer upon another. The system that developed was one in which the creditor himself would endorse the bill and use it to pay one of his creditors. The use of bills to settle debts had been (and still is) widespread in large commercial transactions, but generally only in order to raise cash in a time of need, and generally at a discount. In Lancashire they were used widely by manufacturers for small transactions, apparently for nearly all transactions other than payment of workers (which was done in coin), and with little or no discount, though they were dated for payment some months in the future. Neither the Bank of England notes nor the notes of country banks or merchants were accepted as freely as the bills of exchange. A bill of exchange would pass through many hands, being endorsed and guaranteed by each holder in turn. The confidence of manufacturers and merchants in industrially dense Lancashire was apparently greater in one another collectively than in any one merchant who undertook to specialize as a banker by issuing notes of his own.

II. POLITICAL POWER

The history of economic institutions shows a great deal of change, facilitating economic activity that would have earlier been impossible. No similar development and change has occurred in the political system; yet the need for such facilitation is undoubtedly equally great. To take a simple example of the impact of technological changes on political processes, in nearly all systems of representative democracy, a legislator represents a geographically-defined population unit. But increasing geographic mobility (daily mobility) to and from work, seasonal mobility in the use of leisure, life-cycle mobility in age-specific communities, and residential mobility with changing jobs) creates a fundamentally different infra-structure to society, one which is not based on a geographic unit. Thus geographically-based representation is, or will soon be, an anachronism compared to other systems for effecting the same constituent-representative exchange, systems that have not yet been invented.

Much of political activity consists of transactions that are not radically different in kind from economic transactions. There are, to be sure, many differences, in that politics more often involves coercion and threats of coercion, while economic activity is confined to voluntary exchanges. Yet much political activity is similar to this, and there is much similarity between political power and economic power embodied in money.

An example of an exchange process in politics which has some similarity to economic exchange occurs in the very concept of elected representatives who make policy. The transaction between the representative and his constituents is directly one of exchange: he works to obtain for them policies that will be most satisfactory to them, with the aim of inducing them to reelect him. He has control (with his fellow-legislators) of the creation of policies that are important to his constituents, and he in effect gives up to his constituents that control (by using it to further their interests), in return for which he anticipates their continuing to give to him control over his seat in the legislature, which they can revoke at election time.

The representative system of democracy is based in this and other ways on primitive exchange processes. However, it suffers from numerous defects; one that illustrates its primitive character is that it depends too greatly on the political adroitness of men like Lyndon Johnson or Sam Rayburn, who are in effect like skillful professional traders in a barter economy. And there is a great deal of "looseness" in the system, allowing for political middlemen, like party machines, which can absorb for their own use or divert to interested third parties the value inherent in a vote. In economic systems, the competition of the market, greatly facilitated by a flexible system of money, reduces the possibility of such diversion of value.

How can this looseness, which allows political value to be diverted, be reduced? That is the question I want to examine in this paper, and to begin to answer it by specifying some of the differences between political power and economic money—with the ultimate aim of suggesting new institutions to facilitate political processes.

III. THE CHARACTERISTICS OF MONEY AND OF POLITICAL POWER

The similarities between money and political power have been pointed out by a number of authors. This similarity lies in the major defining characteristic of each: it can be used, within its own realm, to get for its possessor what he wants. The more political power or money he has, the more he can get what he wants in the political or economic realm. Each, in its realm, is the resource which gives control of events or goods.

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[4] See G. Unwin, *Samuel Oldknow and the Arkwrights* (Manchester, 1924), Chap. 12.

The differences between money and political power are, however, great. Some of the principal comparisons are made below.

a. *The range of events or goods that it can control.*

One of the attributes of money is that it can command control over a wide range of goods and services. A quantity of dollars can, within the geographic limits for which dollars are legal tender, purchase any good or service that has a monetary value. Even beyond those limits, it can be used to purchase money which is local legal tender. The geographic range of acceptability of modern money is ordinarily defined by political boundaries and legally defined currency within them. However, in earlier days of noncommodity money, when notes were issued by merchants or goldsmiths, the area of acceptability was defined more by economic trading areas and circles of social acquaintanceship.

It is true, that some goods and services, particularly the latter, cannot be bought. However, in a modern industrial society, the number of these things becomes quite small. In fact, one could well describe the transformation of western society from feudal society to capitalist society as a shift of goods and services from a fixed "privilege" associated with a particular estate or position in society to a good or service that could be bought with a universal currency, money.

Universal power within a given geographic area has not always been characteristic of money. There are many examples in primitive societies of several types of money existing simultaneously, not interchangeable, but used for different transactions. For example, in Yap, Einzig lists nine kinds of money, which were only partly interchangeable: large, (immovable) stone discs, large pearl shells, strings of small shells, dyestuffs, coconuts, tobacco, baskets of taro, woven mats (an intertribal currency), and modern money.[5] In modern society, however, even where multiple currencies exist, there is relatively easy convertibility from one form of money to another. Thus indirectly if not directly, all forms of money have power to command the goods and services that any one of them does.

This universality of control is precisely not characteristic of political power. A given type of political power, for example the vote of a citizen, is limited to control over a specific resource such as a seat in the legislature filled by election. It is valueless if its power is not exercised in that specific election. Similarly, the power of the executive in the United States government is limited to control of specific events. He cannot declare war, nor levy taxes, nor pass laws, nor sentence criminals, nor control any of a number of activities that are outside his circumscribed powers. In other words, political powers are narrowly circumscribed, restricted to control over a single event or a narrow class of events.

The reasons for the difference between money and political power appear to lie principally in the divisibility of the things being controlled. If one has a small amount of money, he can buy a small amount of any type of goods. But one cannot define a "small amount" of political power in this way, because the consequences of such power are not divisible into quantities of various sizes, as are economic goods. "Policies," by their very nature, are relatively universal in application. In economics, they would be described as public goods, for which the consequences for one person are inseparable from those for another person.

This inability to divide political power in the same way as economic power has led to a number of different ways of dividing power to create pluralism of control. When early Rome abandoned kings, two devices were introduced to bring about limitations on political power.[6] One was a *time limitation*, for magistrates were appointed by the Senate for one year only, and not reappointed. The other was a limitation through overlapping realms of power, for *two* magistrates were appointed, each having the same absolute powers, and each having veto power over an action of the other.

In modern political systems, the principal ways of dividing power into "small amounts" appear to be two: (a) a small portion of control over a set of events, as occurs in a collective decision where each member of the collectivity has, through a vote, only a fraction of the control over each decision; and (b) total control over a very limited set of events, as a judge has full control of initial decision over those cases that come under his jurisdiction, but no control over other events in society, except by virtue of his roles other than that of judge. It appears that in democratic societies, form (a) of partitioning of political power occurs for the citizenry, since each has a tiny fraction of control over election of all officials who have direct control of political actions. In contrast, form (b) of partitioning political power occurs for the officials themselves (though for legislators it is a combination of (a) and (b)). Each official has total (or a relatively large fraction of) control over a very restricted set of actions.

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[5] Einzig, *op. cit.*, p. 38.

[6] See F. E. Adcock, *Roman Political Ideas and Practice* (Ann Arbor: University of Michigan, Ann Arbor Paperbacks, 1964).

Another way of describing this difference between political power and money is that political power implies a specific object of power, while money is a generalized resource without a specific object. If one conceived of political power materialized as pieces of paper in a form similar to money, each one of these pieces of paper would have printed on it the object of that power, or the class of events which was its object. A voting slip, for example, would have printed on it the election for which it was valid. Or a piece of paper valid for containing a judge's decision in a case would necessarily be imprinted with a number or title of the specific case.

b. *The negotiability of money and political power.*

A second characteristic of money is that it is not associated with a particular owner. It is fully negotiable, and can be used by the second, third, or fourth holder in exactly the same way as it could be used by its first owner.

Political power does not have full negotiability. If a legislator wants (as often he does) to give up his vote on a particular issue to gain something of greater value to him, the vote still resides with him, though he may engage in an exchange of promises. There is no physical transfer, as in the case of money, but only a promise to cast the vote according to the wishes of the person to whom it was promised. There is, it is true, the case of proxy votes, in which one man gives his proxy to another; and in certain systems, such as stockholders' meetings, proxies may become an important element of power. But even here, the proxy is ordinarily given to a particular person, who himself may not transfer it to another person, so that the chain of transfer is only one or possibly two steps. Also, it is true that only in some collective decisions are proxies allowed. In others, the vote can be cast only by its original owner.

The absence of physical transfer, and the continual attachment of political power to its original owner does not occur in every circumstance; for example; the *delegation of authority* is a transfer of control of certain actions from a superior to a subordinate, and it may occur several times, so that although the ultimate guarantor of the power is the person in final authority (just as the bank is the ultimate guarantor of the value of a banknote), the direct authority over these actions resides with a person far removed from this final authority. Such delegation of authority is always downwards in an organizational hierarchy, and thus not symmetric as is the transfer of money. It is more directly analogous to the transfer of possession of economic goods, which always goes in the direction away from the originator.

There are also less formal transfers of political power which may be in either an organizationally vertical or horizontal direction. For example, the phrase, "passing the buck," refers to a transfer of authority for carrying out an action that has some unpleasant consequences for the one who finally carries it out. It ordinarily refers to transfer of control over a particular action rather than transfer of control over a general class of actions, as in delegation of authority.

These examples, to which many others could be added, indicate that the nontransferability of political power does not as rigidly or intrinsically distinguish it from money as does the broad range of goods over which money has control distinguish it from the narrower range of control of political power. Nevertheless, there are many circumstances, such as the case of a legislative vote cited earlier, in which political power must be exercised by its original owner, and cannot be transferred. Even in those cases where transfer does occur, as in delegation of authority, the person to whom the power is transferred is often seen as the *agent* of the final authority. Thus there is a distinct, if not absolute, difference, between political power and money in transferability, and in its ability to become fully separate from its original owner.

Combining both of the two differences discussed above, it is clear that money exercises its value or power relatively independently of both the subject and the object of the action—that is, independently of the owner of the money and independently of the goods or services over which it brings control. Political power is ordinarily specific to a particular role or status, and is ordinarily limited in scope to a particular object of the power.[7] Thus political power has two specific referents, its subject and its object, while money has neither. Again, if we visualize political power as concretized on pieces of paper, the piece of paper would not only contain on it the specific class of events that constitute its object, but also the name of the position or role in whom that power resides.

[7] Political power in modern society is not, however, associated with a particular person. This separation of power from the particular person and blood lines is a major achievement in the rationalization of society, and is one of the crucial characteristics of bureaucratic authority.

c. *The accumulation of power and money.*

One of the principal properties of money is its ability to serve as a store of value—as a "link between the present and the future," as Keynes says. That is, money preserves the value of one half of an economic exchange until one is ready to carry out the other half. As a consequence of this property of money, it can be accumulated—neglecting for the present the interest-earning character of money—thus making possible large accumulations of wealth under the control of one man.

Political power is different from money in this regard. It does not appear to serve in quite the same way as a link between the present and the future. It appears instead to be a means of distributing, and fixing the distribution of, control over certain actions with public consequences. As such, power cannot be accumulated or preserved for the future. Because the object of power ordinarily is a specific event or class of events, power is time-specific: if it is not used to exercise control over its particular event or action, then it has lost its value. If a legislator fails to vote on an issue, he is not preserving this power, but throwing it away. If a president fails to exercise his power to initiate legislation, he does not accumulate that power. Indeed, just the opposite may occur, if there is not a sharply defined definition of powers: those who do exercise control over the action in lieu of the proper authority may come to assume de facto authority over it which they relinquish only after a struggle.

Just as power is not accumulated, it is not reduced in quantity when it is exercised or used. When a head of an agency or department in a government makes a decision exercising his authority, he does not have any less authority or power after the decision than before. Just as political power cannot, in many cases, be saved, it also cannot be spent. Since political power is ordinarily tied to a specific class of events over a specified period of time, its use does not make the user any poorer.

There is, however, a kind of power which may be accumulated. This occurs frequently in legislatures, where a legislator can preserve the power of his vote on an issue even if he is not otherwise interested in it, by exacting a promise of future aid from someone who is interested in this issue, in return for exercise of his vote in a way that benefits this person. The most adept legislators build up extensive credits or debits in order to concentrate their power on those issues that are most important to them. It is likely that because of the basic time-specificity of power, political credit must be used up fairly quickly, or else it loses its value. It is quite different in this regard from monetary credit, which apart from price changes does not depreciate, but rather gains in value over time, through the accumulation of interest. Thus while money can be described as having a value per unit time, equal to the interest rate at which it is loaned, political credit loses in value over time, and thus political power may be described as having a depreciation per unit time, equal to the rate at which power exercised in the present loses value when exchanged for power to be exercised in the future. Clearly, if money were time-specific in its value, it also could only be loaned at a discount or with a depreciation, simply because its value would be lost if not used at the appropriate time.

There is a direct relation between the ability of political power to be accumulated, and its being used up or spent when exercised. Formal power as laid down in tables of organization or constitutions cannot be accumulated, and neither can it be spent. But power that arises from political exchanges that are payable in the future can be accumulated. Similarly, personal power, in contrast to power associated with a role, can be accumulated and spent.[8] Personal power may arise from simultaneous occupancy of several roles, from having special information or ability to perform in a role, or from an extensive line of credit or reputation that has been built over a period of time.

There is, however, a special kind of accumulation of power associated with roles, through a change in the structure of the political system. When it is said that "power begets power," this usually means that power, the power of a role, can sometimes be used to change the structure of the system itself so as to give that role more power. Such change in the structure of power represents an accumulation of what might be better described as the resources for exercising power. The term power appears to be used in two ways: first as such resources, and second as the actual exercise of control over events.

We may ask why this difference between political power and money, why political power cannot usually be accumulated, while money can be accumulated. It appears that perhaps in this respect the formal power associated with a role is more analogous to the actual productive resources, the productive capital, of an economic system. Such productive resources, which constitute the structure of the economic system, do not often change hands in economic exchange; it is their products that do so. When they do change hands, this is a change in the structure of the system, comparable to a change in the constitution of a political system, or in the table of organization of a bureaucracy. The specific acts through which power is exercised cannot, it appears, be accumulated because they are time-specific, as indicated in the preceding section. The only accumulation comparable to that of money appears to be in political credit, reputation, and informal power, which is often, if not always, associated with a person rather than a role. Thus if systems of power were concretized through the use of pieces of paper or other physical symbols, it appears that we might think of three separate elements:

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[8] When an occupant of a position threatens to resign if he fails to get his way on a given issue, he could do so only a few times at most. This is use of personal power, rather than power associated with the role. Only if a man has filled an office in such a way that his personal occupancy of the position is of value to others does this threat have any power.

(1) the resources for exercising power, which could be concretized as a device for generating a steady supply of pieces of paper, each of which represented the exercise of control over a specific event or action. These productive resources may be conceived as an office or a set of rights as specified in a constitution or table of organization. These resources are analogous to the productive resources, the physical capital, in an economic system.

(2) the pieces of paper themselves, each identified as to the class of actions or events over which it represented control, the position or role to which the power belongs, and the period for which this paper is valid. These are analogous to the products of an economic system.

(3) a second set of notes, or promises to pay, which are issued against the pieces of paper described in (2). These notes are negotiated by the two parties at the time of agreement, and it is likely that the paper described in (2) will either be discounted in issuing these notes, or that there will be a depreciation rate for these notes, or both. These notes are analogous to specie money in an economic system, and would be designed, like money, to facilitate non-simultaneous transactions, to constitute a "link between the present and the future."

It appears that if the system were concretized in this fashion, only the notes (3) can be accumulated in a way that is at all comparable to money; the mechanism (1) is comparable to productive economic capital; and the slips of paper (2), which are comparable to economic products, must either be used or exchanged within their period of validity, or must be discarded.

d. *Scope of legitimacy of power and acceptability of money.*

In every monetary system, the question arises as to what determines the acceptability of money in exchange for goods or services of intrinsic value. Commodity money, which was some widely-traded good, like grain, or, in the South Sea Islands, woven mats, or precious metals exchanged by weight, needed no guarantee of its value, for its value was intrinsic in the demand for the good. The only questions that could arise are ones of liquidity, convenience, and stability of command: how quickly and easily could the money be exchanged for desired goods, how easily was it stored, how stable was its value (i.e., the consumption demand for it in relation to supply). Most of these questions were resolved in the very choice or evaluation that resulted in a particular commodity being chosen: it ordinarily had high liquidity, good convenience of storage and exchange, and a stable relation between supply and consumption demand.

With money that has no intrinsic value, the acceptability of money depends upon the reputation and known resources of the issuer or guarantor. Where there is no single legal tender, that money will be most acceptable which has the most respected and trusted member of the community or institution as its guarantor, and all other money will be discounted against it. Where there is legal tender, as in modern nations, its acceptability also is determined by the confidence of the community in its guarantor, the government. The declaration of one currency as legal tender forces its acceptance, but does not require sales to be made at a given price. If a currency is over-issued, the reduced value of the money can be expressed by higher prices. In a classic case, this occurred in the city of Venice. In order to pay its debts, the city issued, through its bank, about double the amount of money that ordinarily circulated in the city, which was money of the value of about 800,000 ounces of silver. Although the double quantity of money was accepted as payment for the city's debts, prices rose, and after use in a few transactions, they money gravitated back to the city for refund in silver, which Venetian merchants would accept at a premium. In order to stop this, the city had to borrow back about half the notes and remove them from circulation, to maintain the value of the other half—and it found itself in exactly the situation from which it had started. In Germany, at the time of depreciation of the mark after the first world war, the depreciation was so rapid at one point that a worker paid one day would not hold his wage until the next morning, but would send all children out to buy food, before the value had been depressed further. In these cases and others like them, the lack of confidence in the legally-determined money was expressed in an unwillingness to hold money, and in increasing prices.

Thus the definition of money as legal tender does not insure its value; this value is insured only by confidence in the productive resources of the community relative to the amount of money circulating, and thus willingness to hold or accept money (to be exchanged for future products) rather than present products.

The acceptance of money is parallelled by a general class of attitudes called the "legitimacy" of political power. Legitimacy means the acceptance of this power, and willingness to act in accordance with its demands. At times of social upheavals the legitimacy of a given system of power may be brought into serious question by the populace, and unless the power can be backed by direct force, it is overthrown. The legitimacy of power, however, may be confined to a much smaller domain than is the domain of a monetary system. That is, the legitimacy of the power of a police force or a city council or a state legislature or a mayor or a judge does not extend beyond a particular jurisdiction defined by geographic boundaries. The legitimacy of the power of a factory owner does not extend beyond the limits of the organization, and is even limited to certain actions within the organization.

Limitations on the domain of political power are also evident when we turn from formal power to political credit, reputation, and other types of informal power. Though these types of power-credit have more of the attributes of money in that they can be saved, spent, accumulated, and invested, they are like formal power in being restricted to a narrow domain. Since they depend upon the formal power that they derive from, they lose most of their value outside that domain of power. A school principal, with enormous power within his school, has political credit and reputation in the community surrounding it, but is an ordinary man outside his community, with no reputation at all.

One might draw a rough parallel between the early locally restricted monetary systems in use in 18th Century England and the locally-restricted power systems of political units such as towns, cities, counties, and states within a nation. With increasing geographic mobility and increasing relevance of distant events, the power of these local units has become less and less used, replaced by use of power at the national level. A simple example of the general direction of movement is the renovation of cities in the United States. The arguments for and against, and the decision about how much to renovate and how to do so, are made at the national level. The subsequent local decision is little more than a ratification of this action for that locality.

However, despite the drift of attention, decision-making, and general employment of power to the level of nations, there has been no reorganization of the system as has occurred for monetary systems. The result is that this system of collective action functions more poorly than if the appropriate political invention, comparable to the adoption of Bank of England notes, were made.

e. *The unit of measure.*

One final difference between modern money and political power is important. Money has a specific unit of measure, which is comparable from one transaction to another. Political power has no unit of measure, yet political debts are made and paid. Consequently, there must be some accounting scheme for reckoning these debts. The situation has some similarity to primitive barter systems, in which no strict accounting was kept, but a debt of one good was discharged by payment of another quite different in labor-value. There is no reason to believe that our political systems are not at a stage of development similar to that of primitive economic systems which used no strict accounting scheme. It appears more difficult, however, to conceive how a unit of account might evolve in political systems.

The need for a unit of account in economic systems arises only because of the need to allow debts to be settled in general terms, that is, by any of a variety of goods. Without money specified in terms of an abstract value, debt when incurred would have to be specified in any of a number of alternative goods—for if it were specified in only one, then the creditor could always hold the debtor to his original terms, no matter how much their relative value had changed, nor how difficult it was for the debtor to obtain them.

Summary

Five comparisons between money in an economic system and power in an authority system have been made above: (a) the range of goods or events that it can control; (b) the negotiability of money and political power; (c) the accumulability of money and power; (d) the geographic scope of acceptability of money and legitimacy of power; and (e) the quantitative unit of measure associated with money and its absence for power. These comparisons all lead to a common question: how do the specific characteristics of the wants governed by political power constrain the form that such power can take? Clearly a system of power that was parallel in all respects to a monetary system could not be devised, because of the different nature of private goods and public policies. Yet it is equally absurd to believe, as the lack of political innovations seems to imply, that political power must be as different from economic power in its organizations as is the case in existing political systems.

The second part of this paper will attempt to build on the comparisons made above, by examining a number of possible alternatives to current political organizations to see what their most obvious defects might be. One might even go so far as to make a comparison with 18th Century liberalism in economics: in earlier political forms, the production and distribution of private goods was carried out under direct political control, and economic liberalism argued for their removal from such control. This was accomplished with enormous increase in human welfare. Perhaps it is now possible for the production of public goods to be removed from political control.

In examining the possibilities, the distinctions made under (c) above, between the *productive resources* (rights inherent in a role), the *products* (exercise of acts of power by the occupant of a role), and the notes or promises to pay (political credit) are important. It is the last of these, the political credit, which is most analogous to specie money in an economic system, and it is the last of these which is absent in any tangible form, existing only in loose and poorly-accounted agreements. The fundamental difference between the productive resources and products of the economic system, for which money exists as a lubricant, and the productive resources and products of a political (or authority) system, for which there is no analogue to money, is the difference between private goods and public goods. Policies, i.e., public goods, cannot be experienced, or as the economist would say, "consumed," individually, independently, and divisibly, but are carried out (or in economic terms "supplied") for a public altogether, and thus experienced in concert inseparably.[9] In other words, political power is control over certain actions or events that do not have the divisibility of economic goods but have indivisible consequences for all in the political jurisdiction. Economic power is similar control over resources that have the characteristics of private goods and services, that is, they can be supplied and experienced independently. The direct analogue for money does not exist in political power, for money is a device that facilitates economic exchange by providing both a measure of value and a counter for storing the value given in an exchange until needed for use. But in so doing, money itself gains the economic power of the goods whose value it counts and stores. Yet the ultimate economic value, or resources, or power in the system lies not in the money, but in the economic goods or resources themselves. The importance of money for modern economic systems raises the possibility that the absence of such a medium in political processes may greatly inhibit the proper functioning of political institutions, and may, in fact, inhibit political innovations that would aid modern life.

III. NEW POLITICAL FORMS

With the foregoing analysis in mind, it is possible to conceive of a variety of new political forms which would have more of the flexibility that money lends to the economic system. In using that analysis, it is most useful to return first to the distinctions made in section (c) above, between three aspects of an economic system and a political system: (1) the productive resources, (2) the products, and (3) the notes or promises to pay. The comparison is:

	Economic or Market System	*Political or Authority System*
1) Productive resources	1) Physical, intellectual, and organizational capital necessary for production of private goods	1) Rights of control over specific classes of events, as specified by a constitution or table or organization
2) Products	2) Private goods and services offered in a market	2) Actions carried out in accord with rights under (1), having consequences for a given set of persons
3) Money	3) Specie money (promises of a third party) offered in return for control over goods and services under (2)	3) Informal promises offered in return for control over actions under (2)

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[9] This does not exactly correspond to Samuelson's definition of a public good, which involves both the jointness of supply and the non-additivity of con-

With this explicit comparison, it is clear that a major reason that nothing formally comparable to money under (3) exists for the political system is the nondivisibility of acts exercising political power under (2). Any action has a consequence for a given set of persons, so that to exchange control over it to any one of this set may be against the interests of others. To exchange control over it to someone outside this set may be against the interests of all. Thus in any new political forms, careful attention must be paid to the scope of legitimacy of any political money that is introduced, and its convertibility beyond that scope.

Three examples of different forms of power are described below. The first and second involve modifications of a system of representative democracy. The transfer of power from individuals (e.g., citizens) in whom ultimate sovereignty resides, to a body which makes decisions as representatives of these sovereigns is now carried out via elections of representatives by the sovereign individuals, and collective decisions made by these representatives. The first and second examples (a) and (b) below, are designed as modifications of that system. The third, (c) below, is a form of power that contains some of the properties of a vote and some of the properties of money: area-specific currencies with limited convertibility.

a. *Corporate bodies as representatives with fluctuating power*

In political democracies, there is no explicit recognition of the supra-individual actors that have arisen in modern society: corporate bodies in the form of churches, labor unions, business firms, professional associations, and other organized bodies.

These corporate bodies are different from the citizen-sovereigns in one important respect: they have no constitutional political power, but must obtain it from the citizens (as does a trade union in its use of the electoral power of its members to gain the support of legislators), or purchase it with other forms of power, such as money (as does a business firm that helps finance a candidate's political campaign). Consequently, in modern legislatures, these corporate bodies have a very great deal of power that affects the direction the legislature will act.

A different form of political democracy would be one, something like syndicalism, that explicitly recognizes the corporate bodies that have emerged in modern society, and allows them directly to receive from the citizens political support reflecting the citizens' investments in them, and to use that power through votes in collective decisions of that society. In such a form, a citizen would directly cast votes for activities or corporate bodies in which he had the most psychic investment of self.[10] It is these corporate bodies in which the citizens selves have been invested, and it is through the effect of collective actions on the activities of these corporate bodies that the citizen himself feels the effect of political action.

A system of representation in which each citizen casts several ballots for those corporate bodies in which he has investments of self would allow the delegation of political power to directly reflect the self-investments he had made. As these investments change, his votes would go to other corporate bodies. And in all cases, his votes would not be cast for or against a body, but merely for that body. In the representative assembly, the members of the assembly would be the corporate bodies themselves (i.e., their agents), each casting votes in proportion to those the body had received in the election.

The nation and local geographically-based units such as cities and states are also corporate bodies in which individuals make investments of self, so that these bodies as well as others in society would constitute a basis of representation. Because individuals ordinarily make a great investment of self in the nation, the power of its representative would likely be especially great. The representative of the nation as a unit would

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sumption. However, the aim here is not to say that all political power concerns control of the supply of public goods, but rather to give a rough description of the kind of actions which political power controls.

[10] It is Arthur E. Bentley's *Process of Government* (1908) that was most pioneering in this direction, and remains the most theoretically elegant, although it was formulated very loosely. Bentley had a conception of society as composed of *activities,* and of interest groups forming to implement these activities, with the outcomes of governmental actions a resultant of the pressures of these interest groups that were founded on activities. Bentley was never specific about what he included as activities, but it was clear that he intended them to include, but not be limited to, economic activities. In one example he uses, church-going activity is opposed to saloon-keeping activity (with other activities participating as well) over the issue of saloon operation on Sunday. Bentley, however, did not take the next step and examine the possibility that these activities could be the recipient of citizens' votes, and thus represent them in a legislative body. He pays little attention altogether to the form of government, making the implicit assumption that the pressure of the activities through their interest groups would be independent of the governmental form.

attend, in effect, to the interests of the nation as a whole, and insofar as individuals feel that the fortunes of the nation as a nation are important to them, his power would be great. It is obvious, of course, that these psychological investments by individuals fluctuate over time; and as they do, the delegation of political authority in the nation as a whole and in other corporate bodies would fluctuate accordingly.

In a system of representation like this one, every organized activity in society, and thus every corporate body, would compete in a system of pure and open competition for votes of the citizen-sovereigns. The form of the representative assembly might vary from time to time, as the number of different corporate bodies with agents as members varied. Each body would have a number of votes in the representative assembly proportional to the number it had received in the general election.

It is obvious that in modern democracies these corporate bodies wield great indirect power in legislation. It is they that form the lobbies, pressure legislators, write legislation, finance campaigns, organize support for candidates. Thus in the current system they are merely at one remove from formal power in collective action. Yet this remove may make an important difference. Their power to affect the outcome of existing legislative bodies depends on a variety of resources, some of which are not a legitimate part of political currency. Their aims in legislation are often equally hidden, sometimes consisting of private gains at the expense of the general good. Altogether, their actions are hidden from view, and as a "shadow government," as they have been called, they use unknown resources in transactions with legislators, often for private goals. In a body in which their participation was direct and explicit, their resources are those they receive from the citizens through votes, and their activities are less hidden. This does not mean, of course, that other resources such as money do not enter the political arena, for money and psychological investments of people are differently distributed among corporate bodies, thus creating an inherent tension between two forms of power. It does mean, however, that the members of the legislative body would be direct representatives of individual interests, rather than actors interposed between interests and the collective decision, as are geographically-based legislators in existing political systems.

b. *Changes in the income of power to representatives*

In a representative democracy, the constitution or the rules of procedure of the body of representatives provides an automatic income of power to each representative. It does so by giving him a vote when there is introduction of a proposed legislative action. Innovation, however, is possible through modifying this stream of income.

In a collective body that makes only infrequent and irregular decisions, such as a committee that meets only infrequently, this automatic distribution of power coincident with the introduction of a proposed collective action may be the most viable one; but in a body which has frequent collective actions, informal actions reallocate this power through vote-trading and the use of political credit, which concentrate power on a particular action in the hands of those legislators with greatest interest in it. As a consequence, it is clear that some alternative means of providing a power income, which utilizes the same interests that govern the informal transactions, is possible. Such a means of distributing incomes of legislative power should have first the characteristic that power is distributed equally to those in the same structural positions, as it is now, but secondly, the characteristic that power is less fully tied to the specific action than at present, so that legislators could have some option in the allocation of their power income among different issues. Several possibilities meeting these requirements are outlined below.

1) Fixed set of decisions, fungible votes:

If the numbers of bills voted upon in a legislative session were fixed, a legislator could be given a set of votes at the beginning of the session, for him to use on whatever bills he liked. Thus the vote would have associated with it a label indicating only the session for which its use was valid, but not the particular bill within the session. In such a system, the legislator could allocate this income of power in whatever ways he desired, with no necessity of vote trading. Such an arrangement would not violate the interests of his constituents, but merely give him greater flexibility to realize their interests.

The operating characteristics of such an arrangement are difficult to foresee fully. Some points are clear, however, Both on the basis of some observations of a legislative game played in this way, and on the basis of its comparability to an n-person Colonel Blotto game,[11] the legislative session can be expected to be more nearly one of pure conflict. It appears in this respect similar to the situation, in two-person bargaining, in which the move from the initial position to the contract curve has been determined (and thus all the mutual gains extracted), but the particular point on the contract curve is at issue. By making the exchange of votes for political credit that goes on in the legislature unnecessary, since the mutual benefits that are realized in those exchanges are realized at the outset by the more flexible use of votes, much of the non-conflict portion of the activities is removed. It appears that the legislature would become more individualistic, less cohesive, since much of the reason for trust and credit have been removed. However, it is not at all clear whether the end result in legislation would reflect the citizens' interests with more distortion than is the case with issue-specific votes, or with less.

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[11] A Colonel Blotto game is a game in which two opposing players, each with a set of forces, must deploy these forces among several sites. The player with the greatest size of force at a given site wins on that site, and the overall payoff to each player

2) Bank accounts of fungible votes:

In most collective bodies, the number of actions to be decided is indeterminate. In such bodies, the flexibility of the preceding system may be realized by a different procedure. A certain small number of votes is issued to each member at the beginning of the session. He uses these as a bank account of votes, which he can spend on a given issue or save. Then when the first action is voted, and there are s_1 votes cast for the s_2 cast against, the action passes if $s_1/(s_1 + s_2)$ is greater than the fraction determined by the decision rule (e.g., one-half plus one in a majority rule). Then the number of votes cast, $s_1 + s_2$, is redistributed by adding to the bank account of each member $(s_1 + s_2)/n$ votes. A member can thus save votes, if the current issue is not of interest to his constituents, or spend heavily if it is.

Again, the operating characteristics of such a system are not known. Much, but not all, of the reason for vote trading is removed. A member will be able to more easily concentrate votes on actions of greater interest, so that the realization of members' interests (or constituents' interests) through the collective action would appear to be less distorted than with the issue-specific vote and imperfect vote markets. It would seem to have some applicability in a system of direct democracy without representatives, where the membership is so large and dispersed as to make vote trading costly. If each member were allowed to keep an account of votes (with some perishability over time), which he could use on those issues that interested him most, the outcome of the vote in a direct democracy would certainly more fully reflect the differing intensities of feeling than in direct voting with a single vote per issue.

3) Regular vote incomes from constituents:

A third means of providing power incomes to representatives is one in which constituents' votes go directly into a representative's account. Each constituent receives a vote at a fixed interval, say a month. He may cast his vote for or against his representative, during its period of validity. The net number of positive votes received constitutes the representative's bank account to use during this period, on whatever actions he chooses. He recasts the constituents' votes, distributing them as he likes over legislative actions that arise in this period. His votes in the representative assembly in the next period again are the votes cast for him in that period, minus those cast against him. Thus when his constituents are especially interested in the actions likely to arise in the assembly, and when he is casting votes in their interest, they will be very likely to cast a vote in that period. If the first of these conditions do not hold, they will less likely cast a vote. If the second does not hold, they will cast votes against him. In either case, he will have fewer votes to use in his voting.

The operating characteristics of this system are also unknown. It is clear, however, that citizens in a divided district would produce legislators with very little power. However, the effect of a constituent's vote will be much less dependent on the district in which he resides than in current representative systems with elections decided by a majority. The value of the representatives in this case is as vote-concentrators. As is evident, the power of combined votes cast as a unit is in general greater than the power of these same votes cast individually, since when cast as a bloc they can more often determine the outcome.

c. *Mixed money-power systems, and the use of vouchers*

For some goods, which are not divisible finely enough to be distributed according to money held by individuals, mixed systems of power-allocation arise. Ownership of an automobile in a poor country may reside in a collectivity that has been created to provide enough money to purchase the automobile. Ownership of a house is often joint, between husband and wife, and

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is determined by the sum of his gains at each victorious site. This is obviously a game of pure conflict. See R. D. Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley and Sons, Inc., 1957).

often purchased with the money incomes of both. Ownership of a country club may reside in a collectivity of members, with rights of useage determined by vote of the members. Cooperatives often form to purchase recreational facilities, such as beach clubs, and the rights of usage are subject to collective decision. The most widespread mixed system exists in publicly-owned or partner-owned business firms, in which actions of the firm are decided by vote, but partial ownership is purchased by money.

These examples of cases in which goods or activities that are purchasable by money nevertheless come to include also votes as a form of power suggest the basis for dividing power through votes rather than money. When the consequences of an activity are indivisible among the members, and yet each has a claim to some control over the activity, this claim must be exercised by a fraction of control over the indivisible activity, rather than total control over the divisible activity.

There is, in addition, another type of mixed system, involving vouchers in which the activity or good is divisible, but the resources to gain control of it are of restricted convertibility.

Vouchers, stamps, coupons, consumption subsidies, or other devices that entitle the holder to obtain control over a particular activity or good contain one of the attributes of a vote, and one of the attributes of money. (I consider here only those vouchers that are sufficient in themselves to obtain the good or activity, not those, as in periods of rationing, that must be used jointly with money to have any value.) They are, like votes, specific to a restricted activity or good, and have value only for obtaining control over that good. They are, like money, usable to obtain total control over a fraction of the good or activity, not like votes usable to obtain a fraction of control over the total good or activity.

The question arises, then, in what circumstances is this intermediate form of power, which contains a mixture of the properties of votes and money, applicable? Some suggestions may be obtained from those cases in which vouchers are used, or in which their use has been proposed.

Food stamps are distributed in the United States to persons whose monetary power is at a particularly low level. The specific reason for the use of stamps rather than money is the fact that the decision about where the money is to be spent has been made by collective action (through government purchases of food in the price support program). The vouchers serve to distribute a portion of the power that originally inhered in that money, the control of the goods thus purchased, but not a second portion, the choice of whether to purchase these goods or others. The collective decision about the disposition of public money may be made, as in this case, for reasons other than that of restricting the autonomy of the person who ultimately controls the goods (i.e., in this case, for maintaining food price supports); or it may be made specifically to restrict that autonomy.

In a number of European countries consumer subsidies, which constitute a partial voucher for specific goods or activities, are widely used in housing and food for persons with low money incomes. In the United States, public housing or subsidized middle-income housing in large cities is also a partial voucher. In this case, the collective decision which determines these policies is a decision to redistribute a portion of the power that inheres in money to persons with certain characteristics, as in the food stamp plan. However, the decision is explicitly made in these cases in order to withhold that portion of the power of money that inheres in the choice of what class of goods or activities to purchase. The decision is made collectively to augment the economic power of those who have low money incomes, but to restrict this increment of power to those activities or goods determined by vote of the collectivity. The decision is ordinarily made because of a belief that the power given by money will be used for other activities than those in which the collectivity has decided an increment is most valuable.

In other areas, vouchers have been distributed for all members of the citizenry, rather than only for those with low money incomes. Vouchers for the purchase of school services were introduced by some Southern states of the United States, to circumvent the racial integration of public schools. School vouchers have been proposed in England and in the United States for the whole population.

In the area of health, an equivalent to voucher systems has been introduced for all citizens in a number of countries, through a national health service. In these cases, the aim is to redistribute economic power through taxation, but to insure that the funds so redistributed are used for the activity of education or health by redistributing the money in the restricted form of a voucher. Consequently, there is a fractionation of the power of money. The money, publicly held by the government, contains the power to obtain control over various kinds of goods or services. The government uses a portion of that power by collective decision which allocates certain amounts to education, health, etc., and then distributes the remaining portion of the power, that is the rights to use or consumption of the goods or services, among its members. The historical situation in the case of health services is the opposite of that in the case of school services: the introduction of health vouchers in the various national health services was a restriction of the power of individuals, who previously had had the total power of the money whose use is now collectively determined. (There was, of course, also a redistribution of power among individuals). In the case of school services, individuals under the public school system have only the power of consumption of a specific service provided by the government; the use of vouchers is an expansion of that power, to use for any educational service that the individual chooses.

If the total cycle of the production and consumption of the monetary power consumed through vouchers is considered, it consists first of the production of the power through individuals' labor; then the collective removal of this power through taxation to shift ownership of the money from individuals to the collectivity; and finally, a redistribution of a portion of this power through the distribution of activity-specific vouchers.

To make clearer the specific characteristics that differentiate vouchers from both votes and money, it is useful to conceive a voucher system in which the redistributional aspect is removed. Consider a collectivity, a nation with a currency system, that makes a collective decision of the following sort: it determines a number of activity areas, including those activities that are not individually consumed, such as public administration and national defense. It then determines what fraction of individual income payments are to be made in currencies that are usable in various areas of consumption: health, food, housing, recreation, and others, with perhaps a fraction in a currency usable in any area. It does not allow consuming individuals to exchange one currency for another at the government bank, but does allow producing institutions, which receive one type of currency but must pay employees a currency mix, to do so, at fixed rates.

In such a system, the vouchers or area-specific currencies are exactly like money except that their range of power is limited to a specific area. The income earner has lost one degree of power, the power to allocate his income among areas according to his tastes. Within these areas, his income has the full power of money.

This analysis of the specific characteristics of money, votes, and vouchers as forms of power gives some indication of the conditions under which each is most applicable in a social system. Money and votes are clearly applicable when the activity or good is divisible and indivisible respectively. There are some activities in any social system, however, which fit perfectly into neither of these categories. They are consumed by individuals, not by the collectivity as a whole, but the collectivity as a whole has an interest in gaining some power over their consumption. This may be because the collectivity decides they should be supplied equally to all independently of money income, as in health or education, or because the collectivity decides that individual expenditures of income will not allocate expenditures in that area that are commensurate with "real" needs of individuals. (This may be because of market distortions through advertising or other promotion, or merely because individuals do not trust themselves to make rational choices between market areas, when expenditure in some areas brings immediate gratification, while expenditure in others brings delayed gratification.) In these activities, a form of power which is partly at the disposal of the collectivity and partly at the disposal of individuals is clearly appropriate. This is exactly the nature of a voucher or area-specific currency.

It appears likely that in the future development of modern societies, when social production is more divorced from individual productivity, when income payments to individuals for productive activities develop more difficulties as a source of power distribution in society, and as interdependence of activities increases, that a larger portion of the power that arises from productivity will be disposed through such a joint means, involving both decisions by the collectivity as a whole and decisions by individuals as ultimate consumers. The most appropriate emergent form of power as a result of this would appear to be the voucher or area-specific currency.

This discussion of a few alternative forms of political power indicates some of the variations that might exist. What is necessary, in considering the rational design of political institutions, is to consider the essential properties of political power, or control of public policies, as distinct from economic power, or control over private goods—and then to examine the possible concrete forms that political power might take without losing these essential properties. The present essay is intended as a step in this direction.

CREATING POLITICAL REALITY*

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"Suit yourself."
—American colloquialism

Currently fashionable modes of political analysis deserve acclaim today for at least two reasons: they provide opportunities for participating in a pleasurable if strenuous activity (regardless of the value of the end results) and they effectively come to terms with the surface facts of political reality. Our posterity, too, may find it easy to esteem the contemporary products of the profession of political science should it ever look back and see how an affection for craftsmanship is combined with the ability to please. Moreover, the reward system of the profession should appear as having been nicely designed to promote the present display of talent, ingenuity, variety, and success. There is evidence, in any case, that the prevailing inclination to work hard and to develop ever more powerful analytical tools is welcomed and reinforced within the discipline. All would seem to be well.

Yet doubts continue to be expressed today even by those who govern the profession and engage in what Thomas Kuhn has called normal science. Partially, there is a petulant resentment among older practitioners, scholars who are made fretful and irritable by the entrepreneurial opportunism of the *nouveau riche,* by the feeling that mindless industriousness rather than scholarly contemplation is now rewarded by tenure as well as by space in journals, time on panels, positions on editorial boards, and cash for projects. It does not pain me, however, to disregard the indictment that comes from this source—not because I suspect its patrician origins but because I believe it is blind to the underlying impulse of empiricism, because it ignores the subversive, liberating thrust of empirical science. I am also prepared to disregard the assorted indictments which come from *within* the dominant paradigm of the profession: intramural critiques, letters to the editor blown up into articles, manuals by technicians more critical of one another's techniques than of their shared assumptions. It is a third kind of indictment—sometimes little more than an angry, incoherent expression of uneasiness—which I would like to take more seriously.

What has become annoying to political scientists of diverse methodological persuasions is the profession's inability to frame and illuminate the major events of recent times, events which, because they have run counter to expectations, are perceived as critical. And what makes this doubly annoying is that only unaccredited prophets, poets, seers, preachers, and metaphysicians would seem to have foreseen anything like the contemporary crisis in authority. Who within the profession has made our incapacity to govern ourselves—to control the most brutal and the most generous of our impulses at home and abroad—rationally manageable, subject to disciplined statement? One need not turn to the major catastrophes of the age to become uneasy about the way our professional activities are disconnected from our concerns, seemingly trivial matters close at hand having proved sufficient to provoke questions. Previously unseen (or irrelevant) parts of reality have suddenly had the nerve to make themselves visible. Pushing their way into camera range, unrecognized and unrepresented men have fought to be perceived as significant, becoming relevant if not to professional social scientists, then at least to administrators and legislators worried about the possibility of revolution. Submerged groups (students, blacks, women, construction workers, stockholders, homosexuals, clergymen, cops, and even Army recruits, though not yet patients, prisoners, teachers, coal miners, or bureaucrats) have crashed into the mass media and thereby into the arena of politics.

Their emergence (inevitably characterized as an emergency by those within the consensus) has of course been made comprehensible enough on one level: causal analysis has revealed, for example, that the longer and hotter the summer, as one article has solemnly concluded, the more likely the outbreaks of the lower orders.[1] But while causal analysis allows political scientists to cope with new data even in the city of New Haven, while it allows them to explain the appearance of previously eclipsed men or to demonstrate how to engineer their disappearance, the available scientific explanations nonetheless fail to satisfy. Somehow the prevailing modes of explanation do not enable political scientists to comprehend phenomena as fully as the forms used by some novelists, journalists, poets, or film directors. Political scientists—certainly those who have recently turned out in considerable numbers for the special convention panels set up by the Caucus for a New Political Science—vaguely sense that their approach does not permit them to see their subject matter as comprehensively as they might. They do perceive people in motion, but primarily as reflexes and outputs, as effects of causes. Convinced that men must be respected as agents capable of engaging in goal-oriented action, they nonetheless can see little which exhibits purpose, meaning, dignity, and integrity. As political scientists, they are directed to perceive manipulated and manipulatable data; as men with larger concerns and a greater range of compassion, they aspire to perceive something more, namely the public significance of others who happen to be outside the prevailing balance of manifest interests. Professionally, they are impelled—whether by memories of scope-and-methods courses or by the innuendos of their colleagues—to confuse science with an edifice of positivist theory and to identify explanations with unambiguous generalizations about neatly assorted variables. As nonprofessionals, however, they are sensitive to the need to challenge what are alleged to be the real attributes of functional systems.[2]

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* This essay is drawn from a discussion paper prepared for a meeting of several members of the Caucus for a New Political Science at the Center for the Study of Democratic Institutions, Santa Barbara, California, May 1-2, 1969. It was also subjected to helpful criticism by members of the Columbia University Seminar on Political and Social Thought, February 1970.

[1] David C. Schwartz, "On the Ecology of Political Violence: 'The Long Hot Summer' as a Hypothesis," *American Behavioral Scientist,* 11 (July-August 1968), 24–28. In his critique of the Kerner Commission report on civil disorders, Robert M. Fogelson has noted that the Commission, like social

To what extent is it possible to provide professional tools and rewards for such political scientists, for those whose generous moral sensibilities are betrayed by the astringency of their science?

I. A MODEL OF POLITICAL MAN

It is not likely, I believe, that we can expand the focus of political science unless we permit our vision and action to be directed by a model of human nature which allows us not only to account for man's private, economic interests but also to recognize his currently less apparent political possibilities. Unless we learn to acknowledge that more may be present than we have yet experienced, the best political scientists—those most torn between the demands of the discipline and their own moral perceptions, those who bear the greatest strains—will drop out of the profession, becoming increasingly unprofessional and undisciplined. They are apt to follow sentiment, ultimately identifying with their subject matter, arrested and absorbed by its sheer presence, surrendering their critical capacities, becoming so thoroughly engaged by aggregated data that they will finally disdain efforts to bestow structure and form on their experience. If they do not go all the way, they will remain as cynics and opportunists who market the products of normal science in order to be at least free enough for occasional undisciplined, unprofessional forays into the ghettos of our cities and our minds.

To enable them to move coherently—that is, to make it possible for political science to integrate norms and facts, theory and practice, morality and science, ends and means—we will have to ask ourselves whether in our battle against metaphysics, superstition, prejudice, and ideology, we might not have won too large a victory. Frankly accepting the idealist overtones of model building, we will have to reconsider the pathetic directive that scientific models are merely to be checked against the common denominator of present experience and inquire how, instead, we might derive a model of political man from barely recorded, ill-articulated, marginal intimations of human possibilities.

Whatever the difficulties today in accepting the discursive styles generally employed for constructing such a model, it certainly has been recurrently envisaged and formulated. I have had previous occasion to identify its outlines by noting what is shared by Rousseau's self-governed individual, Marx's unalienated man, Nietzsche's self-activated hero, Dewey's educated person, Sartre's man of good faith, Fromm's autonomous individual, and Lifton's protean man. We

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scientists who served it, viewed riots as mere reactions to ills manageable within the prevailing system of liberalism, that is, manageable without redistributing political power: see "Review Symposium," this REVIEW, 63 (December 1969), 1269–1275.

[2] The notion that reality has real attributes is rarely expressed as explicitly as in the following: "Some features of the world *stand out,* almost begging for names. Concepts clouds, thunder, table, dog, wealth, hunger, color, shape, and the like, name differentiated slices of reality that impinge willy-nilly on all of us." May Brodbeck in *Readings in the Philosophy of the Social Sciences* (New York: Macmillan, 1968), p. 3 (italics in original). Similarly, nations, university departments, a market economy, or prevailing role differentiation are recurrently treated as if they were indubitably, imperturbably real.

can give further resonance to these available images of human nature by considering the nonauthoritarian personality of social psychology, the Hermes-like figures appearing in the various myths of the trickster, the picaresque nonhero of modern fiction, or Gordon Allport's portrait of man as a creature in continuous process of becoming. If the result of such an exploration should seem to resemble an unsorted collection of color slides, there is nothing to keep us from carefully sorting them, inquiring how they came to be, and speaking firmly about them with a measure of precision. This would in any case help make vivid a plausible ideal of the healthy personality and might encourage us to inquire into ways for reducing illness.

There is of course a reluctance to permit metaphors borrowed from medicine to orient political life. Won't citizens, unlike physicians, properly disagree on the ends of action? The application of the concept of health to political situations is questioned, in other words, because it would falsely imply agreement on some final ideal, on some static condition.[3] But there is no need to entertain such a simplisitic notion of "health." A healthy system may well be the *end* of medical practice; yet there are good empirical grounds for treating biopsychological systems—including the body politic—precisely as physicians or psychiatrists do, namely as open-ended ones, as systems which are healthy as long as they remain in process.[4] And what is characteristic of human beings in process, of men engaged in political action, is their continuous resolve to display themselves, to express their determination to remain purposefully in motion, to defy necessity and assert as much of themselves as they dare.[5]

That political scientists are actually not innocent of this self-exhibiting, self-promoting aspect of political action is shown by Murray Edelman's current work on poverty as well as by voting studies which allow that voters might conceivably be actors who can be understood in social settings which reveal the expressive character of their behavior.[6] Moreover, there may be an increasing readiness to concede that studies of attitudes and of participation do in practice incorporate unobservable intentions—something more than overt behavior.[7]

The ground for such a perspective, given its classic definition by Aristotle, has been characterized as "political space" by Hannah Arendt. In terms echoing George Herbert Mead no less than Aristotle, she has tellingly identified it as

> the space of appearance in the widest sense of the word, namely, the space where I appear to others as others appear to me, where men exist not merely like other living or inanimate things but make their appearance explicitly. . . . To be deprived of it means to be deprived of reality, which, humanly and politically speaking, is the same as appearance. To men the reality of the world is guaranteed by the presence of others, by its appearing to all; "for what appears to all, this we call Being," and whatever lacks this appearance comes and passes away like a dream, intimately and exclusively our own but without reality.[8]

Man's natural "need," in this view, is the opportunity to *be,* to gain recognition by making witnessed and comprehended appearances and thereby becoming significant to others.

That precisely this—nothing less *and nothing more*—is man's universal need is still hard to acknowledge. It does find implicit support in statements such as the epilogue of Amitai Etzioni's *Active Society,* a thoroughly lucid discussion of human inauthenticity, a term given meaning by his review of authentic basic needs.[9]

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[3] Some of Harold D. Lasswell's overenthusiastic formulations in particular have provoked criticism: see Bernard Crick, *The American Science of Politics* (Berkeley: University of California Press, 1959), pp. 197–209.

[4] See especially Karl W. Deutsch, *The Nerves of Government* (New York: The Free Press, 1963), pp. 139–140; Steven Polgar, "Health," in *International Encyclopedia of the Social Sciences* (New York: Macmillan, 1968), Vol. VI, pp. 330–336; and Marie Jahoda, "Mental Health," in *Encyclopedia of Mental Health* (New York: Watts, 1963), Vol. III, pp. 1067–1079.

[5] As Jeffrey Smith has pointed out to me, it is no less characteristic of developing human beings to strive for a sense of mastery; desiring to master a progressively richer role repertoire, they impose order on play, orgastic releases, and wayward impulses: *cf.* Robert W. White, *Ego and Reality in Psychoanalytic Theory* (New York: International Universities Press, 1963).

[6] See Murray Edelman, "Public Policy and Political Violence," discussion paper, Institute in Research on Poverty, University of Wisconsin, 1968; and Arthur S. Goldberg, "Social Determinism and Rationality as Bases of Party Identification," this REVIEW, 63 (March 1969), 5–25.

[7] One serious effort to show how the dominant paradigmatic directives are being revised is Arthur L. Kalleberg, "Concept Formation in Normative and Empirical Studies," this REVIEW, 63 (March 1969), 26–39.

[8] Hannah Arendt, *The Human Condition* (Chicago: University of Chicago Press, 1958), pp. 198–199; the quotation is said to come from Aristotle, *Nicomachaen Ethics,* 1172b36 ff.

[9] Amitai Etzioni, *The Active Society: A Theory of Societal and Political Processes* (New York: Free

Yet whatever the appeal of such essays, they still respond to the essentialist question of what human needs "really" are. Needs continue to be specified as if some ontological heaven had to be decked out with an assortment of reifications.

To accept the spirit of Etzioni's discussion but avoid its residual Platonism, it should be useful to recall the tradition of empiricism and see it exemplified by Herbert Marcuse's work. His approach is at once far less idiosyncratic than his critics claim and far less complicated than his prose allows.[10] Derived from Hegel, it gives rise to that "unhappy consciousness" generated by the tension between the reality we know all too well and the reality which remains unconfirmed and unrealized—those unacknowledged intimations of life which the prevailing organization of society continues to repress, those unspeakable dimensions of ourselves from which we are kept alienated. Our needs, Marcuse argues with a good deal more fuss than necessary, will make themselves known to us when surplus repression—repression in excess of what men need—is eliminated.

To be sure, we feel that this argument begs the question: just how much repression *do* men need? But the kind of conclusive answer we tend to look for cannot be had within the empirical tradition. We want abstract, unconditional answers whereas Marcuse will rightly provide us only with hypothetical, conditional ones: *if* you reduce repression, his hypothesis states conditionally, *then* the ability of men to harmonize their conflicting drives is likely to be enhanced. Put differently: if reality were changed, men would be more apt to bring diverse experiences into relationships.

But, we might persist in asking, how valid is this proposition? For those empiricists not aligned with the established present, there is only one way to find out. We must proceed by acting as if it were valid: we must be prepared to invest in it, to try. The procedure is of course the familiar pragmatic one. Taking risks, we must attempt to *violate* whatever is alleged to be reality, whatever equilibrium positivists certify to be real. We will then either lose our wager or else compel reality to yield, learning (by doing) that, to the extent that we succeeded in pushing back the coercive forces of the wilderness, we will have gained in manageable experience, in political space. If we win, we will have learned that we did not *need* the prevailing degree of postponed gratification. We will know that there was, in Marcuse's phrase, surplus repression. We had felt we needed to live more amply—and we found out (if we survived) that, yes, we *could*. Clearly, to accept this experimentalism for discovering one's needs is to see how diversionary it is to draw up an abstract inventory of human needs, how such academic exercises function ideologically to keep us in line.

What we need—not what we want—we will only disover in practice, only by treating present systems of domination and necessity as if they used greater violence and more discipline than necessary. Because the surplus benefits elites, only non-elites, outcasts, or unattached outsiders are likely to be motivated to engage in testing, that is, probing in ways not yet legitimated, acting violently.

It cannot quite go without saying that a commitment to testing, to political life as an on-going, self-consciously conducted experiment, poses troublesome practical questions which no abstract theory can presume to answer. Experiments may be so intoxicating or exhausting that they make us frantic or listless, diminishing us in the very process of conducting them. Of course, we can advise (and compel) men to make only such choices which are likely to make their *continuous* action possible; we can direct them *not* to violate others whom they may yet need as irritants for promoting personal growth. The difficulty, we know, is that every course of action forecloses some future options. In short, it is not helpful merely to instruct men to avoid self-destruction and seek self-enhancement. Forced to make specific choices while unable to see far ahead and yet responsible for the *totality* of ourselves (including a future self which includes our present enemies), we are confronted by the most practical problems of priorities and tactics. To reduce the risks of self-destruction while promoting self-enhancement in practice, we surely need all the positive knowledge we can accumulate—whether by drawing on the conditional conclusions of cost-benefit analysis or comparative research. We must seek to know with a measure of certainty what will keep us from violating both our potential self—our present enemies—and those institutions required to realize it.

This pragmatic approach—implicit in Etzioni, radicalized by Marcuse who follows Dewey as

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Press, 1968), Chap. 21. See also Christian Bay, "Needs, Wants, and Political Legitimacy," *Canadian Journal of Political Science,* 1 (September 1968), 241–260; and Peter A. Corning, "The Biological Bases of Behavior and Their Implications for Political Theory," paper delivered at the American Political Science Association meeting, September 2–6, 1969.

[10] For a consistently and hence usefully wrong-headed summary of Marcuse's work, see Maurice Cranston, "Herbert Marcuse," *Encounter,* 32 (March 1969), 38–50.

much as Marx—also sustains Ernest Becker's case for making need satisfaction the test of existing regimes.[11] Though in many ways exasperatingly eccentric, Becker's work seems to me a useful attempt to define alienation in naturalistic terms, moving us beyond institutions and roles that prescribe *needless* destructions of the self. Further, there is Michael Shapiro's current effort to construct a model of man which would be responsive, as he has written,

> to changing needs, values, and interpersonal orientations. Reacting against the way in which conceptions of man have been oriented toward justifying various institutional arrangements, this model of man-the-theorist will be oriented toward exploring man's potential for creating political orders to accommodate changing individual attributes. Such a model, unlike many current formulations, facilitates thinking about directions for changing institutions rather than individual therapy. Empirically, the model focusses on two basic aspects of individual decision-making, information gathering and information structuring. In this way individuals can be assessed in terms of the nature of their decisional postulates. Self-control (the ability to choose) is viewed as dependent upon the extent to which an individual entertains his decisional postulates in an experimental fashion. In this way man can be viewed not only in terms of the closures and reifications he imposes upon his conceptions but also in terms of his potential for learning and development toward forms of social and political organization that are yet unrealized.[12]

Scholarly essays such as these make it possible to see a drive to provide an empirical basis for a conception of man as an open-ended, multi-faceted being, as an actor resisting every certified truth, naturally given to posturing, simulating, performing, playing, equivocating, innovating, testing, improvising—in sum, acting.[13]

Were we to welcome such a view of political man and permit it to work on us at least as an analytical construct, our attention would be drawn toward those presently unknown elements of experience which the newly formulated model would integrate but not assimilate, which it would esteem for being what they are. No doubt, we might hesitate to confront those elements in their fullness, not wishing to see and discuss them publicly. After all, they are still private and taboo, only to be felt and fondled (as Norman O. Brown has insinuated). At best they may be related, like Portnoy's elaborate complaint, to one's analyst in private sessions as privileged communications. Yet if we should find the economic and psychological resources to expand our political consciousness by learning to speak up, we would expose repressed dimensions, gradually becoming aware of those eclipsed parts of life which still fail to live up to our model, which remain in repose, waiting to be exhibited, stimulated, and activated.

Our model, in other words, would serve to alert us to the gap between what we might be and what we are, between political possibilities and the present reality. When this gap makes us sufficiently uneasy, we will unavoidably feel impelled to close it. Our model would therefore do more than create awareness and direct attention: it would induce us to act, to test environments in order to make them yield. To use it would enable us to implement the Kantian view that society is not something "out there" to be studied, but rather, as Etzioni has written, " a human grouping we collectively organize and are free to restructure, within certain limits we seek to understand and untighten."[14]

When we compel reality to accommodate our model of political man—when we are successful both in pretending to *be* such men in sheltered laboratory settings and in extending our laboratories—at least some of the practices and institutions which are commonly alleged to be real will emerge as changeable. As our environments then turn out to be less intractable than we thought (or have been instructed to think) our action will defy and jeopardize the prevailing order of commitments: a functional division of labor, the distinction between private and public sectors, the system of fixed social and biological roles within hierarchical organizations, government by a plurality of elites, the market economy, the identification of security with military power, the separation of means from ends, and finally the organized repression—whether in the family, private associations, or public institutions—of action and pleasure, of politics and play.

Basically, such an approach follows Melvin

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[11] Ernest Becker, *The Structure of Evil: An Essay on the Unification of the Science of Man* (New York: George Braziller, 1968).

[12] Personal communication.

[13] Outside of the traditions of Marxism and pragmatism, it is true, the problem of deriving norms from experience remains. Still, there are at least some discussions which have advanced the arguments about the naturalistic fallacy to a new level; see especially Abraham Edel, *Ethical Judgment: The Use of Science in Ethics* (New York: Free Press, 1955).

[14] Preface to November-December 1968 issue of *The American Behavioral Scientist*.

Tumin by challenging existing captor-captive, superior-subordinate relationships. It questions the structures of power established in schools, prisons, hospitals, political parties, industrial plants, academic departments, professional associations, and nation-states.[15] Postulating political realities dialectically opposed to manifest ones, it negates empirically confirmed experience.[16] The imposing social structures which have been discovered by a non-experimental, non-pragmatic, positivistic social science are thus treated as targets for a political science determined to test their value—their value always in the relation to the structure of human needs. Political science thereby comes to terms with whatever men in power insist is given—given by providence or merely by the process of history, the wisdom of the founding fathers, the necessities of industrialism, the iron law of oligarchy, the immutable nature of man, or the discoveries of a half-hearted empiricism more intent on finding answers "out there" than on honoring its own critical, negativistic, reality-defying spirit.

II. TESTING "REALITY"

If some form of political science is to restructure our sense of reality—to give new meanings to life by engaging in what Dewey called social reconstruction—what are the relevent procedures? How is "reality" best tested to make it satisfy basic human needs?

Before urging the obvious—namely, the extension of programs for experimentation, simulation, prototyping, and participation—I should note that non-participation has assumed at least two forms, and that we have been warned, I think to excess and for the wrong reasons,[17] only about the one which involves the construction of empirically empty frameworks, mathematical systems, and formalistic analytical models. Anti-behavioralists might well acknowledge that there are worse activities than efforts to provide abstract designs whose exhaustive meaning is their public existence, that the kind of abstractions welcomed in the arts—John Cage's resounding silences or white painted on white—need not be rejected in the social sciences. The more insidious form of non-participation is that of irony. Having previously written on the uses of irony (and having left matters less than clear), I would merely like to remark—not argue—that the ironic mode should be regarded as appropriate only when nature is as intractable as Sisyphus found it or else when it is infinitely pliable, only when nothing whatever can be done about our fate or, alternatively, that all is so well that we can actually disarm, play freely, and reverse roles at will.

Attracted by the ironic posture, we tend to overlook that it demands an inhuman suspension of judgment—the refusal to discriminate, the determination to accept nothing as finally serious. Unintimidated by their humane sentiments, the practitioners of irony have of course given us some stunning accounts: Tocqueville's account of equality, Weber's of bureaucracy, Schumpeter's of capitalism, Riesman's of the lonely crowd, Veblen's of the higher learning, Galbraith's of the techno-structure, Lasswell's of the garrison state, Huntington's of the military virtues. And from the perspective of our survey research centers, individuals have suddenly emerged as if they were characters of modern fiction. In Harold Kaplan's words—which refer to Dreiser, Dos Passos, and Farrell, and not to Campbell, Converse, Miller, and Stokes—

> . . . we are impressed by the fumbling irrelevance in the subjective consciousness of their characters. They are portrayed in terms of rather simplistic laws of psychological and social behavior, while their own consciousness is groping, penetrated by only brief glimpses of light. The field of vision is at the extreme of externality, so preoccupied by the super-personal design of social and biological process that the agents within it seem mindless.[18]

Torn out of context—removed from their authors' impassioned moral concerns—these accounts of institutions and individuals exhibit a capacity for icy detachment, for objectivity and disinterestedness. The posture is serenely nonpartisan. There is no interaction with the phenomena, no experimental intervention—only rigorous clinical passivity, observations made in cold blood. Because their authors are manifestly uninvolved, their work enables us to see more of life than we normally permit to fall within our range of vision. The ironist magnifies and thus makes visible what we are otherwise reluctant to recognize—our fixations, compulsions, addictions, and limitations, our behavior. He removes us from ourselves—at least our familiar active selves—and shows with barely concealed pleasure how little we amount to and how well we behave. At the same time, he unwittingly shows himself to be someone who condescends to examine his data. His very prose reveals his self-restraint: controlling his sympathy and his rage, he displays his superior power—the power of aloof observation. To understand him, to sympathize with him, we need merely recall Truman Capote standing by in cold blood (as we still do) when the State of Kansas proceeded to execute the objects of his study.

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[15] See Melvin Tumin, "Captives, Consensus and Conflict: Implications for New Roles in Social Change," in Herman D. Stein (ed.), *Social Theory and Social Invention* (Cleveland: Case Western Reserve University Press, 1968), pp. 93–113.

[16] See Warren G. Bennis and Philip E. Slater, *The Temporary Society* (New York: Harper & Row, 1968); Orion F. White, "The Dialectical Organization: An Alternative to Bureaucracy," *Public Administration Review*, 29 (January–February 1969), 32–42; Theodor W. Adorno, *Negative Dialektik* (Frankfurt: Suhrkamp, 1966).

[17] It is not always acknowledged that social scientists engaged in mathematical enterprises are also participants. Seeking not to *explain* but to express meanings inherent in alternative structure of behavior, they play their roles in plain view of other members of their scientific community.

[18] Harold Kaplan, *The Passive Voice* (Athens, Ohio: Ohio University Press, 1966), p. 9.

Yet an exclusive commitment to such a posture—ever tempting for social scientists who aspire to gain status by separating emotion from fact—is not justifiable, as I have said, unless intervention is futile and absolutely nothing can be done to satisfy our diverse human needs. During such desperate times, we may rightly prevent despair and assign ourselves some activity, exercising what freedom remains, diverting ourselves and escaping necessity. We may then observe and leave a record. When nature (or the state) is implacable and the end is near, that may be the only way to leave one's mark.

To be sure, *no* case, as we fear late at night, is other than terminal. But to go on, to save what we can, we may nevertheless pretend to the contrary and mock reality. Careful probing of reality may suggest that we still do have options, that some of our needs—most basically our need to live, to amplify our existence, to communicate —may still be satisfied. When this becomes evident, we have no warrant for irony. We are then called on (not by some abstraction but by our needs) to integrate the role of observer and actor, of scientist and citizen, to join ethics to policy, norms to facts, and theory to practice.

It should be apparent at this point that action to bridge the gap between man as a value-creating being and man as a value-neutral analyst of the predicaments of others will necessarily be symbolic (as all action is) and, more specifically, that for social scientists it is likely to be literary, that is, ideographic and discursive, capable of elaborating new contexts to reveal the limits of the old. The accounts of the political scientist—his markings, his symbols—will be derived from what he believes to exist beyond accredited reality—beliefs he comes to express and hold by virtue of his experience, his sympathetic involvement.

But if his literary act can scarcely be said to "explain," if his claims are not verifiable because they do not refer to experiences others are ready to observe, on what grounds can we possibly accept his testimony?

I would urge an effort to recover the oldest of tests, the one Aristotle applied to drama: our performances are successful insofar as they enable us to comprehend the polarities of our existence, leading us to take fuller cognizance of them, inducing us to identify visions and interests which, when unidentified and merely experienced, terrify and paralyze.

We will *know* that an author's work is successful only in practice, only as he demonstrates his ability to establish more parts of ourselves and our environment than we had dared to admit into our presence. The critical and wholly empirical question is to what extent his symbols have moved us to confront and accept progressively more complex realities. To the charge that he might simply lie, there is but one response, namely that he assuredly does, that at least so far the truth has not been told. What is at issue is not the fact that he makes things up—depriving "reality" of its success—but, as Nietzsche noted, the quality of his fabrications. To what extent do they make a greater variety of experience manageable? Do they embody a maximum range of bearable interests?

Still exploiting Nietzsche, I would have us assent to ventures which serve to multiply meanings and create new possibilities, which appropriate new space and new time. The newly created realities must demonstrate that the established ones do not have an exclusive claim to the political arena. The new ones must supplement or negate the prevailing ones. They will have to compete with the great hit plays (plays experienced as real enough, however, by anyone involuntarily involved in them) put on by corporate boards, university administrations, welfare bureaucracies, National Guard units, peace research centers, or other institutions for crisis management. Successful hit writing teams, as Donald Fread has noted in defending his own play, "Inquest," have staged performances such as

> "The Gulf of Tonkin" and "The Black Panther Will Get You if You Don't Watch Out." And there's the play called "Why Don't They Take a Bath?" "The Body Count" is a wonderful comedy of the absurd. . . . There is also that wild satire by Sidney Hook and other liberal humanists who have told us to use the democratic process. . . . There is the story of "The Outside Agitator," the great continuing adventure series which began with the Palmer raids. Occasionally you have a new star, like the

wife of Attorney General Mitchell. . . .[19]

At issue for a political science determined to offer counterrealities is not the morality of whatever skits and plays happen to be staged but the likelihood of their expanding the political life of those affected by them. A reality-creating venture is of value to the extent that, *without destroying self-awareness,* it leads men toward increasingly complex realms of being, freeing them to be progressively more playful and political, more active and alive. But the final test, one social scientists might tentatively wish to apply first to doctoral dissertations, must be its impact on the author himself: does the very process of ordering experience give his life greater measure of meaning? Does it enable him to gain access to additional dimensions of himself?

Although I have been referring to literary acts, there is no reason for political scientists to confine their activities to them. Physical spaces are no less stages for action. Ever since Machiavelli pointed the way, we have in principle been prepared to treat societies, too, as artificial creations, quite deliberately designing them so that they might maximize opportunities for developing our capacities, so that man-made institutions might support as much variety and conflict as we can bear without being overwhelmed.

If the political scientist's familiar environment fails to move him—if it strikes him as inert, dull, fatuous, or torpid—he might therefore seek to become the author of more complex social enterprises, rearranging society, doing violence to it by probes, tests, irritating questionnaires, and disconcerting research designs until it provides him with more stimulating material. Accordingly, he will be violating and restructuring social settings, creating synthetic spaces for experimental purposes, encouraging new things to happen in what had been stable, well-manged enclaves—whether these are welfare bureaucracies, university departments, or professional associations. His purpose is not, however, to bring about some supposedly desirable state of affairs from which men will eventually benefit. It is to increase public knowledge of public possibilities in the very process of public action. Engaged in what Robert Rubinstein and Harold D. Lasswell have called prototyping,[20] he will seek to make a conspicuous record of his interactions, leaving as coherent an account as he can of the meanings of each of his successful probes.

The experimental probing I am urging might well begin modestly at the very places political scientists find themselves at work—their classrooms, departments, institutes, or colleges. Were each of these arenas to be treated lightly—as if they did not quite amount to what is proclaimed by cause descriptions or catalogue prose, as if they held unfulfilled promises for participation —they would be saved from easy success. And were action to be accompanied by efforts to publicize the experimental process, we would be offered practical demonstrations of theoretical possibilities.

In this view of the activities of political science, the practitioner attempts to *express* what he is doing, interweaving theory and practice. Aware of his own operations, seeing them as others might so as to communicate his new experience, he uses words to circumscribe what in life remains slippery and inconclusive. He thus makes experience accessible to himself and to others. In a word, he politicizes it.

This procedure is but analogous to the experiments of those contemporary artists whose work, as the present director of the Guggenheim Museum has noted without misgivings, "is impossible for collectors to collect, for museums to show, for dealers to handle, for critics to appraise. . . ."[21] *The new constructs aim at including the spectator, at bringing in the outsider.* They are at once more embracing and more fragile than the constructs of political scientists.

Should political scientists proceed to transform hierarchical, goal-oriented administrative systems into equalitarian, process-oriented political ones, they would be engaged in the kind of artistic enterprises which impel us to become aware of ourselves insofar as we have been enabled to fill spaces, invent alternatives, impose perspectives, and establish connections. Should political scientists subject themselves to the discipline required to complicate and break up stable social environments, they would create vulnerable structures which remain embarrassingly open and which, like pregnant pauses, demand participation. They would then embrace empiricism fully and invite distrust of even their own conclusions—indeed all completed work. Their very tone would then be that of the novelist as narrator who has given up the omniscient voice. They would decline to speak as authorities—in effect following *Finnegans Wake* which, as Charles Newman has noted, is "the first great book without a single sentence we can *trust*."[22]

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[19] Quoted in *The New York Times,* May 3, 1970, p. D-9. I have argued for appropriating more of reality in "Expanding the Political Present," this REVIEW, 63 (September 1969), 768–776, and in "Making Scenes," *Massachusetts Review,* 11 (Spring 1970), 223–255.

[20] See Robert Rubenstein and Harold D. Lasswell, *The Sharing of Power in a Psychiatric Hospital* (New Haven: Yale University Press, 1966).

[21] Their characteristic work, as Thomas M. Messer has pointed out, cannot be accommodated by galleries; moreover, it is perishable, if not self-destructive, "It is aimed against—or rather, it implicitly denies—the channels between artist and viewer, as well as the entire intermediary machinery consisting of dealers, critics and museums. What results is not simply opposition to the order of the day, but rather the creation of a dimension that renders such an order irrelevant and anachronistic." Thomas M. Messer, "Impossible Art—Why It Is," *Art in America,* 57 (May-June, 1969), 30–31; see also the telling illustrations assembled by David L. Shirey, *ibid.,* pp. 32–47.

Those who have invested in the prevailing social order are understandably anxious about the ambiguous and therefore untrustworthy features of every new structure. Screening project proposals or research applications, they are reluctant to support scholarly activities which promise no more than a wider sharing of the satisfaction of participating in social experimentation. For them, this promise is hardly enough. They expect unambiguous payoffs in the form of conclusions reached. Yet when pledged to an interactionist, anti-positivist paradigm of political science, we must necessarily view such a demand for specifications as misguided. Unable to claim advance knowledge of our findings, we can at best merely pretend, seeking support for our enterprises by operating on the principle, as David Bakan has said, of being "one ahead":

> To be "one ahead" means to apply for a grant for a piece of research that one has already conducted, and to use the money thus obtained to conduct further research. When one thus works "one ahead" it is possible to write rather splendid applications. Cynical as this approach may appear, it is actually sounder from a scientific point of view than conducting research based on so-called good design. For the fact of the matter is that good research into the unknown cannot be well designed. . . .[23]

To be sure, since we do have empirical knowledge of conditions for "good research into the unknown"—requisites of innovative activities—we can request support for promising approaches. But just as Marx declined to discuss the classless society, we should not go further and identify the functions of future social arrangements. We should have been warned by that obsessive preoccupation with detail and affection for logistics characteristic of utopian thinkers from Henri de Saint-Simon to G. D. H. Cole, realizing that our problem is to live up to the epistemological demands of a transactional paradigm by making both knowledge claims and future experiences conditional on action. There is no alternative but to resist all requests from foundation officers to blueprint and illustrate and specify what it would mean finally to create new political realities, what it would mean to expand the political present and bestow political qualities on non-political time and space. We simply cannot *know* what it would mean until the moment our resources are exhausted, until our research grant has been spent—when, presumably, our application for the next grant is already being processed.

Given this persuasion, the political scientist's concern cannot be with learning something from what he and his colleagues have done but with accentuating and intensifying the very process of acting. The rewards for him and others come not at the end of the game—in the form of answers, results, conclusions, findings, predictions, explanations, or hypotheses confirmed—but in action itself, in the curiously gratifying knowledge that one's ability to maintain one's balance under stress is being continuously tested—and that one has not, at least not yet, succumbed.

To gain in sympathy for such balancing acts, perhaps it is sufficient to remind ourselves that openness is in fact a long-standing convention of empirical science—in fact characteristic of all institutions which reject finalities by compelling men to review their conclusions, interrogate one another, and add to their burdens. Perhaps it is necessary (if not sufficient) to welcome what Sheldon Wolin has recently rejected: the near-religious skepticism of behavioralists which has "abolished all privileged beginnings," their "animus against tradition," their faith in a "self which has been purged of inherited notions."[24]

The professional demands made by such a radicalized empiricism—one which constitutes what Herbert Simon has called the sciences of the artifical—are obvious enough. Committed to the openness entailed by valuing man as self-governed agent and repelled by procedures and organizations which frustrate us *needlessly*, we are forced to identify practices which fail to serve our natural needs—specifically our need to take turns in playing diverse, mutually incompatible roles. And as we become sensitive to whatever forces arrest our development, we may learn to appreciate educational institutions which move men, in Marx's phrase, "to do one thing today and another tomorrow" without ever compelling anyone, if all goes well, ever finally to embody merely one of his various functions.

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[22] "Beyond Omniscience: Notes toward a Future for the Novel," *TriQuarterly*, 10 (Fall 1967), 37–52; I am grateful to Paul F. Kress for alerting me to the relevance of this article.

[23] David Bakan, *On Method* (San Francisco: Jossey-Bass, 1967), p. xiv.

[24] Sheldon S. Wolin, "Political Theory as a Vocation," this Review, 63 (December 1969), 1068.

III. EXPANDING POLITICAL EXPERIENCE

I have been asserting, in sum, that a model of political man as value-creating being would serve to direct attention to current nondecisions (or at least to incomplete ones) and that it would induce us to transform present organizational structures and attitudes. Moreover, I have maintained that in order to create new meanings and to construct new realities, we should avoid detached ironic postures and instead seek to interact with our environment, whether (1) by offering descriptive accounts which stimulate both reader and writer, which activate phenomena and dramatize more of life than had been immediately present, or (2) by experimenting in practice, thereby demonstrating the satisfaction of achieving not some end-result but of being engaged in expressive activities. The final test of both of these frankly provocative, intrusive strategies lies in their power to bend what is alleged to be unbending, to expand political experience.

Becoming aware of the desirability of engaging in either vicarious or actual experiments may be all that is feasible today. As we proceed to employ obtrusive measures, we may recurrently find ourselves thwarted (even if consoled by textbooks which teach the uses of unobtrusive research techniques). Unable to be effective even while stationed near the levers of power, we may learn that action is futile and agree with Raymond Williams "that if we move at all we put at risk every value we know, since all the actual movements, all the actually liberating forces, are caught up in a world of reciprocal lying and violence, with no point of entry for any sustained action. . . ."[25] But even though recurrent defeats may teach us that this is true, it still remains possible to define our political situation and make its pathos explicit. If these are not happy times, they may nevertheless be partially redeemed by our being generous with words and calling them what, in the end, we found them to be.

To accept this strategy requires welcoming scientific modes which do more than dignify the vulgar successes manifest at the center of our public life. It requires extending ourselves, surrendering some of our hard-won methodological principles, and seeking to incorporate the flotsam and refuse now at the periphery of consciousness. The proper procedure—my reiterating the point in a new context may now help make it more plausible—is necessarily experimental and discursive, for we reach unknown, unnamed territory only by discoursing, advancing ideas as scouts, sending our words out to appropriate new worlds, using our language to relate inchoate experience to what is established, well-formulated, and familiar. Were we to value this use of the imagination as a distinctive discipline, we would move lightly and playfully through new territory, receptive to its promise, proceeding (as in life) in ever-widening circles. We would at the same time remain prepared to retrace our steps so as to avoid more trouble than we can tolerate. Retreating when necessary, gaining confidence by being as verbose and repetitious as I have been, willing to publish variations of our essays again and again, we would ultimately demonstrate that the new territory can be managed; that it has been secured, and that we can risk moving ahead.[26]

There is no point in documenting the obvious and showing how effectively the conventional diction and present organization of political science discriminate against such reality-creating, expresssive action.[27] A pragmatic activism finds few legitimate outlets within the normal curriculum. Defying paradigmatic expectations, such activism is perceived as undisciplined and irresponsible. It is seen to reduce political science (like physics at its best) to the level of the humanities. Even worse, it leaves us exposed to the gravest of all charges—that we are unrealistic. Our problem, I think, is somehow to accept the charge and not plead guilty. This will entail not only affirming that it is all right to outwit reality but also demonstrating that the escape from necessity gives pleasure. It will require treating one's own work—indeed the whole of political life—quite seriously as a form of play.

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[25] Raymond Williams, "Parting of the Ways," *Commentary,* 47 (February 1969), 73–75.

[26] What my metaphor intimates may be understood as aesthetic education: see Reginald Snell, "Introduction" to Friedrich Schiller, *On the Aesthetic Education of Man* (New Haven: Yale University Press, 1954); Herbert Read, *Education through Art* (London: Faber, 1943); John Dewey, *Art as Experience* (New York: Minton, Balch, 1934); and Herbert Marcuse, *An Essay on Liberation* (Boston: Beacon Press, 1969), Chap. 2.

[27] Pointless activities—such as providing examples of the obvious—may of course be gratifying. Consider John C. Harsanyi, "Rational-Choice Models of Political Behavior vs. Functionalist and Conformist Theories," *World Politics,* 21 (July 1969), 524–526, as an example of a plea for perceiving symbolic, expressive action as mere "preoccupation." Harsanyi would have us treat symbolic action alternatively as excessively costly, as "result of neurotic or psychotic conditions," as "a way of making a social commitment" to achieve results, or as desperate acts by "extremist" groups which "have given up all hope of achieving their goals by reality-oriented instrumental activities, and so feel they have nothing to lose by turning to magic, ritual, ideology, and other forms of symbolic behavior."

Were we to reconceptualize politics itself as play, we would be led to recognize the prevailing modes of analyzing and diagnosing political behavior as ideological, serving those who reserve the pleasures of play for themselves. We would be led to suspend our heavy rhetoric and attempt a lighter tone, risking ambiguities and puns, arrogantly speaking of political reality as if it did not exist, placing quotation marks around our most precious possessions—"the individual," "private property," "deterrent credibility," "the free world," "the democratic process," "political science," and—why not?—"reality." We could thus indicate that these marvels are "so called," that we have made them all up and that—given wit and passion, courage and luck—we might yet remake them to suit ourselves.

A CAUSAL APPROACH TO NONRANDOM MEASUREMENT ERRORS

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The purpose of this paper is to examine several specific kinds of nonrandom measurement errors and to note their implications for causal model construction. In doing so, my secondary purpose is to sensitize the reader to the crucial importance of making one's assumptions fully explicit and to the advantages of a causal models approach to measurement errors. It is well known that the presence of even random measurement errors can produce serious distortions in our estimates, particularly whenever one is attempting to assess the relative contributions of intercorrelated independent variables.[1] Nevertheless, common practice is to utilize what Duncan refers to as the naive approach to the presence of measurement errors: that of acknowledging the existence of measurement errors, and even discussing possible sources of such errors, while completely ignoring them in the analysis stage of the research process.[2] That is, measured values are inserted directly into causal models as though they adequately reflect the true values. It can easily be shown that such a practice, while leading to important simplifications, can readily lead one astray. In particular, it may blind the analyst to searching for alternative plausible explanations that allow for measurement error.

There have been a number of very recent papers in the sociological literature, some of which will be briefly summarized since they may not be familiar to the reader.[3] For the most part, these papers have dealt rather systematically with ways to handle *random* measurement errors, whereas nonrandom errors have been dealt with only incidentally and much less carefully. One approach to nonrandom errors that will not be discussed in the present context is exemplified by papers by Duncan and by Siegel and Hodge.[4] In brief, the strategy is to construct causal models of very specific substantive problems and to insert various reasonable estimates of measurement errors based on outside evidence or plausible assumption. The implications of the various alternatives can then be traced through the causal system to see what practical differences can be derived from the alternative assumptions. My own approach will be to attempt to outline a strategy that can be applied more generally and that might be combined with the Duncan and Siegel and Hodge strategies in concrete applications. Specifically, the approach that will be discussed in the remainder of this paper requires that one make explicit assumptions about the sources of nonrandom errors and that one have available multiple indicators of each variable that has been imperfectly measured.

Even random measurement errors lead to numerous complications because of the fact that they introduce additional unknowns into the causal system. Often this leads to underidentified systems that cannot possibly be solved unless the ratio of measured to unmeasured variables is increased through the use of multiple measures. As we shall presently see, the existence of nonrandom errors of measurement can create a hopeless situation, even with multiple measures, unless the sources of nonrandom errors can be located and measured, or unless these sources are completely uncorrelated with other variables in the theoretical system. As we consider some of these complexities, it is hoped that the discussion will sensitize the reader to the necessity of investing substantial time and thought in the data *collection* stages in order to minimize the effects of disturbing influences. We shall see that there is relatively little that one can do in the analysis stage unless the sources of nonrandom error can be recognized and incorporated into the model.

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[1] For example see J. Johnston, *Econometric Methods* (New York: McGraw-Hill, 1963), Chap. 6; Robert Gordon, "Issues in Multiple Regression," *American Journal of Sociology*, 73 (March 1968), 592–616; and H. M. Blalock, "Some Implications of Random Measurement Error for Causal Inferences," *American Journal of Sociology*, 71 (July 1965), 37–47.

[2] See O. Dudley Duncan, "Contingencies in Constructing Causal Models," in Edgar Borgatta (ed.), *Sociological Methodology 1969* (San Francisco: Jossey-Bass, 1969), Chap. 3.

[3] See especially Herbert L. Costner, "Theory, Deduction and Rules of Correspondence," *American Journal of Sociology*, 75 (September 1969), 245–263; and David R. Heise, "Separating Reliability and Stability in Test-Retest Correlation," *American Sociological Review*, 34 (February 1969), 93–101. See also George W. Bohrnstedt, "Observations on the Measurement of Change," in *Sociological Methodology 1969*, *op. cit.*, Chap. 4: David E. Wiley and James A. Wiley, "The Estimation of Measurement Error in Panel Data," *American Sociological Review*, 35 (February 1970), 112–117; and H. M. Blalock, "Multiple Indicators and the Causal Approach to Measurement Error," *American Journal of Sociology*, 75 (September 1969), 264–272.

[4] See Duncan, *op. cit.*, and Paul M. Siegel and Robert W. Hodge, "A Causal Approach to the Study of Measurement Error," in H. M. Blalock and Ann B. Blalock (eds.) *Methodology in Social Research* (New York: McGraw-Hill, 1968), Chap. 2.

Before discussing nonrandom errors it will be necessary to review briefly certain approaches to the elimination of purely random errors. We shall then consider two important kinds of nonrandom errors: (1) those that are functions of the variable being measured, and (2) those that are produced by one or more variables that may or may not be related to the other variables in the causal system.

I. RANDOM MEASUREMENT ERROR

If measurement error is purely random, such as one might expect to occur as a result of careless responses, coding, or punching, we may write down a very simple model linking the measured and unmeasured variable by the equation $X' = X + u$, where X is the true value, X' the measured value, and u the error component. If u is truly random, then its mean should be zero (except for sampling error), its variance σ_u^2 should be unrelated to X (homoscedasticity), and its population covariance with X should be zero. If it is a resultant of a large number of minor causes that are weakly intercorrelated, then the distribution of the error term should also be approximately normal, though we shall not find it necessary to assume the normality property in the discussion that follows. The essential feature of this simple model is the assumption that X and u are uncorrelated. This property makes it possible to express the variance of X' as the sum of the variance of X and the measurement error variance:

$$\sigma_{X'}^2 = \sigma_X^2 + \sigma_u^2 \qquad (1)$$

Thus the variance of the measured value will be greater than the true variance of X by the amount σ_u^2 which of course can never be negative.

This relationship between the variances of X and X' implies that the ordinary least-squares estimate $b_{YX'}$ of β_{YX} in the equation $Y = \alpha + \beta_{YX} X + \epsilon$ will be biased downward according to the approximate formula

$$E(b_{YX'}) = \frac{\beta_{YX}}{1 + \sigma_u^2/\sigma_X^2} = \frac{\sigma_X^2}{\sigma_{X'}^2}\beta_{YX} \qquad (2)$$

where $E(b_{YX'})$ refers to the expected value of $\beta_{YX'}$.[5] There are a number of practical implications of eq. (2). Since the degree of attenuation in the slope estimate is proportional to the *ratio* $\sigma_X^2/\sigma_{X'}^2$, it follows that even though σ_u^2 remains constant, attenuation will be a function of the absolute amount of variation in X. Whenever σ_X^2 is reduced relative to σ_u^2, we can expect the slope estimate to be further attenuated. Thus if one were to introduce control variables in such a way as to reduce σ_X^2 he would be led to infer, in the presence of random measurement error, that the slope of the relationship between X and Y was being reduced. Likewise, two investigators working with different populations would be likely to obtain different slope estimates, even with the same measurement accuracy.[6] Random measurement error in X will likewise produce an attenuation in the correlation between X and Y. Random measurement error in the dependent variable Y will attenuate the correlation coefficient but not the slope estimate.

A number of ways of taking out or correcting for this attenuation effect have been suggested. Wald and Bartlett, among others, have suggested a grouping procedure that involves grouping individuals according to their X' scores, placing all those with high X' scores (say, in the top quartile) into group 1, and those with low X' scores (say, in the bottom quartile) into group 2.[7] Denoting the group means for X and Y by $(\bar{X}_1, \bar{Y}_1)$ and $(\bar{X}_2, \bar{Y}_2)$ we can then form the very simple estimator

$$b = \frac{\bar{Y}_2 - \bar{Y}_1}{\bar{X}_2 - \bar{X}_1} \qquad (3)$$

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[5] Johnston, *op. cit.*, 149–150.

[6] See Blalock, "Some Implications of Random Measurement Error," *op. cit.*

[7] See Abraham Wald, "The Fitting of Straight Lines if Both Variables are Subject to Error," *Annals of Mathematical Statistics*, 2 (1940), 284–300; and M. S. Bartlett, "Fitting a Straight Line when Both Variables are Subject to Error," *Biometrics*, 5 (June 1949), 207–212. For an excellent discussion of these and related approaches, as well as an extensive bibliography, see Albert Madansky, "The Fitting of Straight Lines when Both Variables are Subject to Error," *Journal of the American Statistical Association*, 54 (March 1959), 173–205.

It turns out, however, that a crucial assumption underlying the procedure is that the method of grouping cases must be independent of the measurement error in X. Since the true X scores will be unknown, and therefore grouping must be carried out by using X' instead of X, this assumption will be violated in practice. Using Monte Carlo procedures, Carter and Blalock have shown that the Wald-Bartlett estimates appear to have biases that are approximately equal to those of ordinary least squares.[8] Therefore it would seem as though these grouping procedures are of little practical value in instances where random measurement errors are nonnegligible.

A second approach to correcting for random measurement errors in X, the so-called method of instrumental variables, requires one to locate direct or indirect causes of X that are not causes of Y (except through X).[9] More formally, we must find one or more instrumental variables Z_i that are reasonably highly related to X, that do not appear in the equation for Y (which may contain several other independent variables), and that are uncorrelated with the disturbance term ϵ in the equation for Y. In the case of the bivariate relationship $Y=\alpha+\beta_{YX}X+\epsilon$ and a single instrumental variable Z, it can be shown that under the assumptions of the model the estimator

$$b^*_{YX'} = \frac{\Sigma(Y-\bar{Y})(Z-\bar{Z})}{\Sigma(X'-\bar{X}')(Z-\bar{Z})} \qquad (4)$$

will be a consistent estimator of β_{YX} (i.e., one having negligible biases in large samples). In effect, by taking a ratio of *covariances* of Y and X' with Z we get around the difficulty produced by the fact that $\sigma_{X'}^2>\sigma_X^2$.

The instrumental-variables approach can readily be generalized to the multivariate case. However, it is relatively more sensitive than ordinary least squares to certain kinds of specification errors. In particular, if Z is a cause of Y through some path other than through X, then Z should explicitly appear in the equation for Y. If one erroneously assumes a model in which Z is omitted from the equation and used as the instrumental variable, then the instrumental-variable estimator $b^*_{YX'}$ will have greater bias than the ordinary least-squares estimator.[10] Therefore the instrumental-variables approach requires an a priori theory of the underlying causal structure and should ordinarily be used in conjunction with other procedures to be described below.

Drawing on the work of others who have stressed the importance of multiple measures and corrections for attenuation, Costner and Heise have developed path analytic procedures that enable one to handle random measurement errors whenever there are multiple measures of each variable.[11] Heise deals with instances where there are single measures at three or more points in time, whereas Costner's procedure requires two or more measures of each variable at a single point in time. Both procedures can readily be generalized and, if combined, can also be used to infer the existence of certain simple kinds of nonrandom errors.[12] In the present paper, I shall confine my attention to the Costner approach which will be extended in later sections. We shall assume that data have been collected at a single point in time, though in many instances it will be helpful to have data at two or more points in time. Costner's approach can be generalized to k-equation recursive systems, though I shall confine my attention to simple two- and three-variable models.

The basic approach can be exemplified in the simple model of Figure 1, which contains two unmeasured variables X and Y, and two indicators of each variable. The absence of other arrows leading to X_1 and Y_1 indicates that there are no sources of measurement error other than purely random error. (Throughout this paper, I shall for the sake of simplicity omit all arrows from diagrams unless such arrows are needed to indicate sources of correlation between pairs of variables.) The path coefficients (here, all correlation coefficients) connecting all variables are represented by the p_i, with the direction of influence being given by the appropriate arrowhead.

A path coefficient is a standardized regression coefficient and can be interpreted as being the ratio of two standard deviations. In the denominator is the actual standard deviation in the dependent variable, whereas in the numerator we have that portion of the standard

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[8] L. F. Carter and H. M. Blalock, "Underestimation of Error Variance when Employing Wald-Bartlett Slope Estimation Techniques: A Monte Carlo Simulation," *Journal of the Royal Statistical Society* (in press).

[9] For discussions of instrumental variables see Johnston, *op. cit.*, 165–166; Carl Christ, *Econometric Models and Methods* (New York: John Wiley, 1966), 404–410; and Carter and Blalock, *op. cit.*

[10] *Ibid.*

[11] See Costner, *op. cit.* and Heise, *op. cit.*

[12] See H. M. Blalock, "Estimating Measurement Error using Multiple Indicators and Several Points in Time," *American Sociological Review*, 35 (February 1970), 101–111.

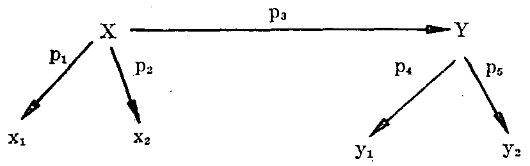

FIG. 1

deviation in the dependent variable that can be attributed to the particular independent variable under consideration, with the remaining causes controlled.[13] Such standardized measures have certain disadvantages that make it difficult to interpret them across populations or from one time period to the next, but this question need not concern us here.[14] As Wright has noted, however, standardized measures have advantages in instances where one is dealing with unmeasured variables.[15]

The attractive feature of path analysis in the present context is that it provides a handy device for writing down expressions involving the unknown p's in terms of the empirically obtained intercorrelations among indicators. It is assumed that the reader is reasonably familiar with the rules for writing down such expressions by inspection of a path diagram. In the case of recursive systems involving one-way causation, and where there are no curved double-headed arrows connecting exogenous variables, the rule for tracing paths is as follows: we may write the total correlation between any two variables as a sum of simple and compound paths obtained by tracing (a) forward from one variable to the other, (b) backward, or (c) backward and then forward. We cannot, however, trace forward and then back since this would involve moving through a common effect of two variables. In complex models it is preferable to work algebraically, rather than tracing paths, because of the likelihood of either neglecting certain compound paths or including them more than once.

In the case of Figure 1 there are four measured variables X_1, X_2, Y_1, and Y_2 and therefore there are six intercorrelations among these indicators. In connection with X_1 and X_2 we note that there is only one compound path connecting these indicators, namely p_1p_2. Similarly, there is a single path $p_1p_3p_4$ connecting X_1 to Y_1. Proceeding through all possible pairs of indicators we obtain the following:

$$\begin{array}{ll} r_{X_1X_2} = p_1p_2 & r_{X_1Y_2} = p_1p_3p_5 \\ r_{Y_1Y_2} = p_4p_5 & r_{X_2Y_1} = p_2p_3p_4 \\ r_{X_1Y_1} = p_1p_3p_4 & r_{X_2Y_2} = p_2p_3p_5 \end{array} \qquad (5)$$

Notice that we have six equations but only five unknown p's. Therefore we have one redundant equation that must be approximately satisfied by the data if the model is in fact appropriate. That is, if measurement errors are truly random the model implies that the product $r_{X_1Y_1}r_{X_2Y_2}$ should approximately equal the product $r_{X_1Y_1}r_{X_2Y_2}$ since (within sampling error) both are equal to the expression $p_1p_2p_3^2p_4p_5$. However, since it turns out that certain particular kinds of nonrandom errors can also be shown to give this same prediction, this test criterion should be considered a necessary but not a sufficient condition the data must satisfy if the random measurement error model is to be retained.

If the model is retained, one can then solve for each of the path coefficients by simple algebra. For example

$$p_3^2 = \frac{p_1p_2p_3^2p_4p_5}{p_1p_2p_4p_5} = \frac{r_{X_1Y_1}r_{X_2Y_2}}{r_{X_1X_2}r_{Y_1Y_2}} = \frac{r_{X_1Y_2}r_{X_2Y_1}}{r_{X_1X_2}r_{Y_1Y_2}} \qquad (6)$$

and

$$p_1^2 = \frac{p_1^2p_2p_3p_4}{p_2p_3p_4} = \frac{r_{X_1X_2}r_{X_1Y_1}}{r_{X_2Y_1}} \qquad (7)$$

If there were more than two indicators of each variable, then under the assumption that all errors are random, only two new path coefficients would be added to the system, whereas the six measured indicators would give rise to fifteen intercorrelations. This would produce an excess of eight equations that the data would have to satisfy if the model were to be retained. Also, with additional indicators it becomes possible to test for and estimate certain simple kinds of nonrandom errors.

In the event that the indicators of each variable are "equivalent" (having the same means, standard deviations, and correlations with the underlying variable), it becomes possible to reduce the number of unknown p's and to increase the number of redundant equations. For example, if so-called "parallel forms" of a measuring instrument had been used in obtaining X_1 and X_2, then one might reasonably assume that $p_1 = p_2$. Or if Y had been measured by the same instrument at two points in time, and *if* one were willing to assume no real change in Y or contamination effects of the first measure, then one might set $p_4 = p_5$. Corrections for attenuation discussed in the testing literature are based on this particular kind of simplifying assumption.[16] Heise provides a method for separating out real change from measurement reliability based on this kind of simplification. The combination of multiple indicators with panel data also makes it possible to solve for certain kinds of nonrandom measurement error components.[17]

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[13] For discussions of path analysis in the social science literature see Raymond Boudon, "A New Look at Correlation Analysis" in Blalock and Blalock, *op. cit.*, Chap. 6; O. Dudley Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, 72 (July 1966), 1–16; and Kenneth C. Land, "Principles of Path Analysis," in *Sociological Methodology 1969*, *op. cit.*, Chap. 1.

[14] See especially John W. Tukey, "Causation, Regression and Path Analysis" in Oscar Kempthorne *et. al.* (eds.), *Statistics and Mathematics in Biology* (Ames, Iowa: Iowa State College Press, 1954), 35–66; and H. M. Blalock, "Causal Inferences, Closed Populations, and Measures of Association," *American Political Science Review*, 61 (March 1967), 130–136.

[15] See Sewall Wright, "Path Coefficients and Path Regressions: Alternative or Complementary Concepts?" *Biometrics*, 16 (June 1960), 189–202.

II. MEASUREMENT ERROR A FUNCTION OF X

As soon as one allows for the possibility of nonrandom measurement errors he must begin to face a number of difficult problems, both conceptually and methodologically. How can such nonrandom errors be represented in mathematical equations in a realistic way? Obviously, one must have certain ideas as to the sources of such nonrandom errors and the kinds of distortions being produced; otherwise the situation is too vague to handle. But in many instances it is even difficult to specify exactly what one means by the "true" value of X.

Let us consider two examples. On the one hand, we can readily imagine a "true" percentage of persons who voted for President Nixon, or even the true percentage who "intended" to vote for him. If a survey consistently underestimated this true percentage in every state, or if it underestimated it for males but overestimated it for females, then we would say that measurement error was nonrandom. On the other hand, consider a variable such as the "true" amount of political alienation in the United States. Lacking a theoretical definition of alienation that clearly specifies the research operations, it is even difficult for us to think in terms of measurement biases or nonrandom errors. Yet we would want to design an instrument that does not systematically exaggerate the alienation of blacks while underestimating that of whites. The notion of nonrandom measurement errors makes some sense in this instance, but it is difficult to pin down because of inadequate theoretical conceptualization. Unfortunately, many of our most important concepts in the behavioral sciences are more like that of alienation than that of a voting percentage.

In the case of alienation, if we could imagine a "true" value X, we might be pleased if we could obtain a measure X' that is an exact linear function of X, say $X' = a + bX$, with no error term. The coefficient b would measure an error of scale, whereas a would give the intercept error. If there were some meaningful zero point (no alienation), then we would prefer that the intercept error be zero, so that we could use the properties of a ratio scale. But in tests of causal models, or in estimating path coefficients, we would encounter no special difficulties if $a \neq 0$. We would have measured X perfectly except for an exact linear transformation. Of course if we wished to report a measure of central tendency, such as the proportion intending to vote for Nixon, such errors of scale and zero point could be disastrous practically, and we would want to be assured that $X' = X$.

In the present context, let us simplify our analysis by ignoring the problem of errors of scale and intercept by taking $X' = X + e$. However, we shall permit the error term e to be some function of the true X and a random component u, and we shall find that we thereby reintroduce a scale and intercept factor as a result of this error component. This seems to be a relatively straightforward way of conceptualizing the measurement error problem without introducing undue simplicity. Although the error component may be a nonlinear function of X, as we shall illustrate below, we shall confine our actual analysis to the linear situation where we may write

$$X' = X + e \quad \text{and} \quad e = c + dX + u \qquad (8)$$

where u is a random variable with expected value zero, with variance σ_u^2 independent of X, and with cov $(X, u) = 0$.[18] We may then write

$$X' = c + (1 + d)X + u \qquad (9)$$

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[16] For further discussions of this point see Bohrnstedt, *op. cit.;* Heise, *op. cit.;* and Siegel and Hodge, *op. cit.*

[17] See Blalock, "Estimating Measurement Error," *op. cit.*

[18] There is another possibility that we shall not discuss further, namely that the error *variance* σ_e^2 is a function of X. Sometimes it is reasonable to take $\sigma_e^2 = a + bX$, where $b > 0$, in which case it may be advisable to transform X to log X. However, this may produce nonlinear relationships with other variables in the causal system.

Since X and u are uncorrelated we have

$$\sigma_{X'}^2 = (1+d)^2\sigma_X^2 + \sigma_u^2 \tag{10}$$

Notice that if d is negative it is entirely possible for $\sigma_{X'}^2$ to be less than σ_X^2.[19] This means that if ordinary least squares is used to estimate β_{YX}, we will generally obtain a biased estimate, but we cannot count on this bias being in the downward direction. Also, it turns out that the method of instrumental variables cannot be used to obtain a consistent estimator. The factor $(1+d)$ will appear in this estimate, and since d will be unknown there appears to be no purely statistical device for eliminating its influence in the derived estimate. In effect, then, the d coefficient will be confounded in slope estimates relating X to other variables.

Before examining the implications of this kind of complication for path analysis, let us consider some substantive examples for which we might anticipate that the measurement error component is a function of X. We shall later discuss instances where the error is a *correlate* of X due to the operation of extraneous variables, but we are now considering instances where the level of X itself has a direct or indirect effect on the measurement error component. As we begin to conceptualize our measurement errors in causal terms, I believe we shall begin to see the picture more clearly than is at present possible. Nevertheless, there are several kinds of instances for which this particular model is reasonably plausible.

Some Examples. One obvious possibility is that the true value of X may be consistently underestimated or overestimated by a constant proportion of the actual value. For example, persons may report their ages to be .9 of their true ages or their incomes to be 1.25 times their true incomes. Somewhat more realistically, such fractional representations might be appropriate for certain subpopulations (e.g., people over 21, or white collar workers). Whereas differential proportions applied to various subpopulations would imply *nonlinear* relationships between the measurement error component and X, the situation might reasonably be approximated by a series of linear models applied to diverse subpopulations.

Perhaps a more common phenomenon is that of regression effects due to measurement error. Of course if one is dealing empirically with fallible measures and purely random measurement errors, he will encounter a regression effect of a somewhat different nature. For example, if at time one we measure individuals with random error, then certain persons with intermediate true values will appear extreme because of the random errors. But with the next measure, they will be likely to regress toward the mean, leaving us with the impression that there is a homogenizing influence at work. However, there will be other individuals who appeared intermediate at the first measurement whose second scores will have diverged from the mean, again due entirely to random measurement error. A true homogenizing influence can be distinguished from random measurement error effects, in this instance, by a comparison of the variances of the measured values at both points in time. If the regression effect is an artifact of random measurement errors, these variances should be approximately equal, whereas if there has been a homogenizing effect, the variance at time one will be greater than that at time two.[20]

We are concerned with a different kind of regression effect, however, namely that due to nonrandom errors that are negatively related to the true X scores. Suppose there is a norm operative to the effect that it is desirable to appear "moderate" or "normal." If respondents are given a series of questions designed to tap political conservatism, one might then expect that ultra-conservatives will try to appear only moderately conservative, whereas radicals may wish to appear liberals, and liberals as moderates. If increasing values of X represent increasing conservatism, then the measured scores of conservatives would be less than the true scores, and measured scores of liberals would be greater than the true values. In other words, the slope d linking e to X would be negative.

Of course the norm might not operate equally in all subpopulations, and might even favor polarization, in which case liberals might wish to appear radical and conservatives as ultra-conservatives. In this case the slope linking e to X would be positive and $(1+d)$ would be greater than unity. Again, if different norms operated in different subgroups a more complex model would be required, and measurement error would be taken as a joint (nonadditive) function of X and other variables. As a first approximation, however, linear models with different error slopes could be used. Our principal difficulty in these and other instances is a lack of theory concerning specific sources of nonrandom errors.

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[19] An empirical example where this occurs in connection with floor and ceiling effects has been discussed by Siegel and Hodge, *op. cit.*

[20] For further discussion of regression effects see Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research* (Chicago: Rand McNally, 1966).

Another source of errors producing regression effects may arise in the process of aggregating micro-level data, although once more the mechanisms involved seem poorly understood. Suppose, for example, that one has a social psychological theory that on the individual level the number of Negroes to whom a white individual is exposed will affect his political attitudes concerning the role of the federal government in school desegregation issues. But the measurement of exposure to Negroes would necessarily be very indirect and perhaps indexed by the percentage of Negroes in the local area. But how big an area? Living within a mile radius? Within the same county? The same state? The larger the unit selected the less adequate the measure of exposure, and also the less the variation in percentage Negro. That is, whereas counties may range in minority percentage from zero up to almost one hundred, if states are used as the aggregate units the largest percentage may be in the neighborhood of 45 per cent. Likewise, at least in the South, there are no states with under five per cent Negro. In effect, the range of variation in X, per cent Negro, will be reduced whenever larger aggregates are used, and the measure of "exposure" will be biased toward the mean. As larger and less homogeneous aggregates are used, one would thus expect the slope $(1+d)$ to approach zero unless other mechanisms were simultaneously at work. Obviously, sources of nonrandom error such as those produced by aggregation have not been adequately studied by political scientists and sociologists.

As an example of measurement errors that are more complex functions of X, let us consider possible distortions produced by so-called floor and ceiling effects and by measurement instruments that are relatively insensitive to variations in extreme scores. Obviously, if there is a true floor or ceiling effect, then measurement errors can appear only in a single direction or else they would be recognized as nonsensical and thereby modified. If "per cent urban" were measured as negative or greater than 100, an error would immediately be noted. In fact, it would be impossible to obtain such scores by conventional means of measuring per cent urban. But if the true per cent urban (by whatever definition) in a county were very close to zero, measurement errors would usually be in the positive direction. Similarly, these errors would be in the negative direction at the upper limit. Assuming

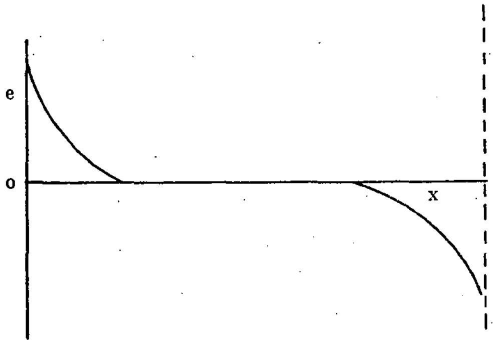

FIG. 2

approximately random errors in intermediate positions, we might expect that the function relating e to X might be approximately as in Figure 2. Such a function might be approximated by a cubic equation, or by three straight lines, the middle one of which has a horizontal slope. Where the floor and ceiling effects merge into each other imperceptibly, this measurement error situation might be approximated by a single straight line with a slightly negative value for the error slope d.

A very common practice whenever measurement is clearly recognized as being crude is to resort to a relatively small number of ordered categories. In the extreme case the analyst may use dichotomies in order to simplify his analysis. It may not be recognized that such very simple procedures produce both random and nonrandom measurement errors that become increasingly serious as the number of categories is reduced. Of course it will ordinarily be difficult if not impossible to estimate the extent of such measurement errors, but this fact does not miraculously make these errors disappear. It merely makes it impossible to assess their distorting effects. In order to understand these effects it will undoubtedly be necessary to conduct a series of Monte Carlo studies on data with known properties, using various methods of categorization in diverse causal situations. Here it will be possible to provide only a single illustration.

Suppose that the true underlying distribution of X is approximately normal, with a mean of 50 and a standard deviation of 10. We then know the proportion of cases expected to fall within any particular interval. Now suppose that a social scientist, ignorant of the true distribution, obtains an approximate measure using a scale that enables him to divide the population into six ordered categories, as indicated in Figure 3. Ordinarily there will also be random errors of measurement that can re-

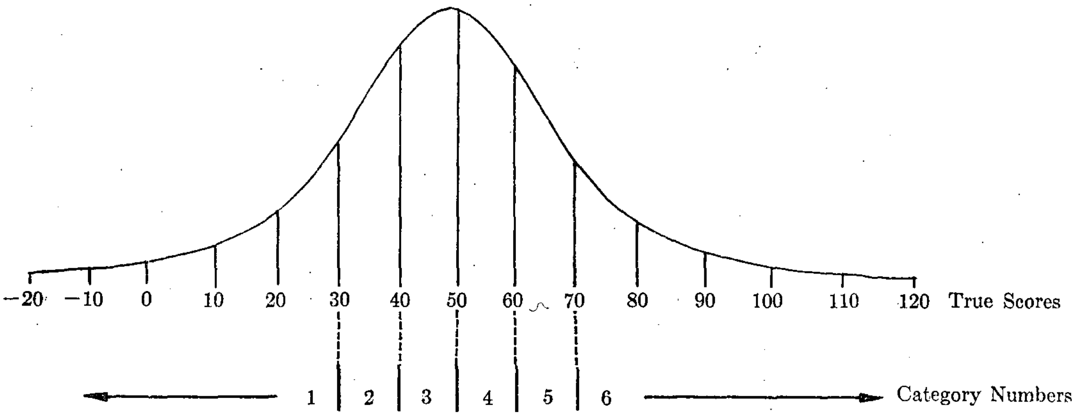

FIG. 3

sult in misclassification superimposed upon effects produced by the fact that individuals in the tails of the distribution will all be lumped together in the extreme categories.

Ignoring the random errors of classification, let us examine the nature of the measurement errors that would be produced if the analyst simply assigned scores of 1–6 to his categories and then proceeded to analyze his data as though no measurement errors existed and as though he had a legitimate interval scale.[21] The X' scores therefore range from 1–6. Let us say that the true X scores range from −20 to 120. We thus have a change of scale in addition to any measurement errors, but this can be handled by dividing each X by 10, the width of the analyst's intervals, and then subtracting 1.5. Thus a true score of 50 is converted to an X' score of 3.5 which is equivalent to the average score of the two middle categories. We can then obtain the "measurement error" components at selected levels of X from the equation $e = X' - X$ as follows:

True X	$(X/10-1.5)$	X'	e	True X	$(X/10-1.5)$	X'	e
−20	−3.5	1	4.5	50+	3.5	4	.5
−10	−2.5	1	3.5	60−	4.5	4	− .5
0	−1.5	1	2.5	60+	4.5	5	.5
10	− .5	1	1.5	70−	5.5	5	− .5
20	.5	1	.5	70+	5.5	6	.5
30−	1.5	1	−.5	80	6.5	6	− .5
30+	1.5	2	.5	90	7.5	6	−1.5
40−	2.5	2	−.5	100	8.5	6	−2.5
40+	2.5	3	.5	110	9.5	6	−3.5
50−	3.5	3	−.5	120	10.5	6	−4.5

For example, an individual with a true score of −20 would be placed into the first category and would receive an X' score of 1, whereas his adjusted X score would be $-20/10-1.5 = -3.5$. Therefore the "measurement error" component e for this individual would be $X'-X=4.5$. As we proceed down the columns we note that e decreases as the true X increases until we reach the boundary of the second class interval at $X=30$. For any individual having a score slightly less than 30 (symbolized by 30− in the table), the X' score will remain 1. In-

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[21] It might seem absurd for an analyst to treat this kind of data as an interval scale, but we must remember that ordinal data also involve equally difficult problems of interpretation. Notice that in this particular illustration we are assuming no additional distortions in the intermediate values produced by the numerical values assigned to the categories. That is, the true distance between category limits is always ten. If the analyst used unequal intervals that were scored 1–6, there would be the added complication of nonlinearity in this intermediate range. Obviously, methodological studies of the practical effects of these kinds of distortions are very much needed. For an example of one such study see Sanford Labovitz, "Some Observations on Measurement and Statistics," *Social Forces*, 46 (December 1967), 151–160. See also, Thomas P. Wilson, "Critique of Ordinal Variables," *Social Forces* (forthcoming).

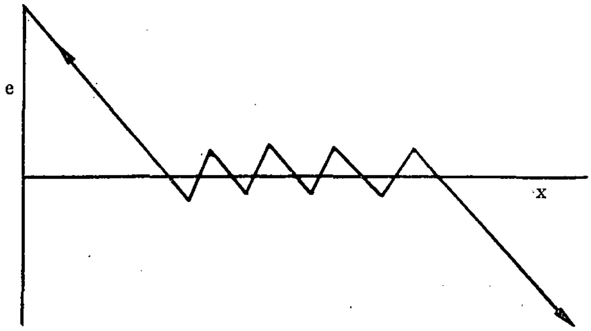

FIG. 4

dividuals whose scores are slightly over 30, however, will be placed in the second category (assuming the absence of random errors), so that for these individuals the error component $e=2-(30/10-1.5)=+.5$. This jump in X' values near the category limits produces a saw-tooth effect, as indicated in Figure 4. This effect continues until we reach the upper boundary of category 5, at which point the values of e decrease linearly as X increases. When we plot the measurement error e against X, as in Figure 4, we note that, except for the saw-tooth effect, the relationship is very similar to that indicated in Figure 3 for floor and ceiling effects.

In practical terms, this kind of distortion can be reduced by using a large number of categories and by making sure that the extreme categories contain relatively few cases. If this has been done, and if one is willing to consider distortions producing the saw-tooth effect as approximately random, then one may safely ignore the extreme effects and use a random measurement error model. But if only a small number of categories have been used, or if there are numerous cases in the extreme categories, the measurement error should be taken as a negative function of X. Hopefully, a linear approximation may be reasonably adequate.

Path Analyses with Linear Models. Let us assume that a linear approximation is appropriate and that we wish to assess the implications of measurement errors that are functions of X. We have already seen that if $X'=c+(1+d)X+u$ we must anticipate a change in scale that will affect our estimates of regression coefficients. But what about path coefficients or correlations? Will our causal inferences be affected, apart from the matter of scale? Common sense would suggest that the same tools appropriate to random measurement error may be used, but common sense is a notoriously poor guide in complex situations such as this. If we represent the nonrandom component as a function of X, we may construct a causal diagram in which X' is taken as a function of two variables, X and e, with e in turn being a function of X, as indicated in Figure 5. Furthermore, we may construct relatively complex models allowing for multiple indicators and several imperfectly measured variables.

Let us consider the model of Figure 6 involving two indicators of X, both of which have been measured with errors proportional to X, and a dependent variable Y likewise measured with nonrandom errors by indicators Y_1 and Y_2. We may ask whether it is possible to estimate the path coefficient p_7 connecting the true values of X and Y in the presence of so many unknowns. Clearly, there are more unknowns than equations, but it does not therefore follow that some of these unknowns cannot be estimated.

Since there are four measured variables there will be six intercorrelations, the path equations for which are as follows:

$$\begin{aligned}
r_{X_1X_2} &= p_1p_4 + p_1p_5p_6 + p_2p_3p_4 + p_2p_3p_5p_6 \\
&= (p_1 + p_2p_3)(p_4 + p_5p_6) = p_1^*p_2^* \\
r_{Y_1Y_2} &= p_8p_{11} + p_8p_{12}p_{13} + p_9p_{10}p_{11} + p_9p_{10}p_{12}p_{13} \\
&= (p_8 + p_9p_{10})(p_{11} + p_{12}p_{13}) = p_3^*p_4^* \\
r_{X_1Y_1} &= p_7(p_1 + p_2p_3)(p_8 + p_9p_{10}) = p_7p_1^*p_3^* \\
r_{X_1Y_2} &= p_7(p_1 + p_2p_3)(p_{11} + p_{12}p_{13}) = p_7p_1^*p_4^* \\
r_{X_2Y_1} &= p_7(p_4 + p_5p_6)(p_8 + p_9p_{10}) = p_7p_2^*p_3^* \\
r_{X_2Y_2} &= p_7(p_4 + p_5p_6)(p_{11} + p_{12}p_{13}) = p_7p_2^*p_4^*
\end{aligned}$$

where we have been able to utilize the compound paths

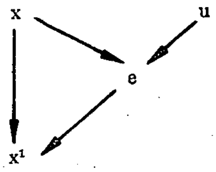

FIG. 5

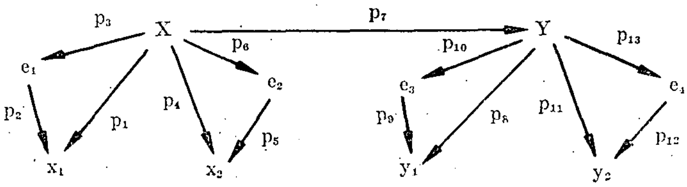

FIG. 6

$$p_1^* = p_1 + p_2p_3 \qquad p_3^* = p_8 + p_9p_{10}$$
$$p_2^* = p_4 + p_5p_6 \qquad p_4^* = p_{11} + p_{12}p_{13}$$

Notice that each of these compound paths p_i^* involves three unknowns that always appear together in identical expressions. In effect, there are two excess unknowns in each of these compound expressions, thus accounting for eight of the unknowns in the system. Although none of these four compound paths can be decomposed without additional data or assumptions, it is possible to estimate p_7 and each of the four p_i^* from the six equations. Of particular interest is the estimate of p_7 obtained as follows:

$$p_7^2 = \frac{p_1^*p_2^*p_7^2p_3^*p_4^*}{p_1^*p_2^*p_3^*p_4^*} = \frac{r_{X_1Y_1}r_{X_2Y_2}}{r_{X_1X_2}r_{Y_1Y_2}} = \frac{r_{X_1Y_2}r_{X_2Y_1}}{r_{X_1X_2}r_{Y_1Y_2}}$$

This estimate is of course identical with the estimate obtained earlier in connection with Figure 1 (with different subscripts). Furthermore, we note that we obtain the same redundant equation for testing purposes, namely the prediction that $r_{X_1Y_1}r_{X_2Y_2} = r_{X_1Y_2}r_{X_2Y_1}$. In other words, Costner's technique works exactly as in the case of purely random measurement errors. However this means that Costner's procedure and test criterion cannot be used to ascertain whether measurement errors are purely random or whether there is an additional error component that is proportional to X. It can be shown that we could have also estimated p_7 if we had had a perfect measure of Y instead of two indicators. Had there been only a single measure of Y involving measurement error, however, we could not have estimated p_7 without additional assumptions.

It should again be cautioned that path coefficients and correlations will not be invariant across populations or time periods, even in instances where the same fundamental laws are operative (i.e., where the same unstandardized coefficients hold). Therefore in instances where one wishes to compare coefficients in the presence of variances that differ from one population to the next, the above procedure will not be appropriate. Also, in order to be assured of reasonable sampling errors in the case of these rather complex expressions, one must be dealing with large samples and correlations of at least moderate size.

III. MEASUREMENT ERRORS DUE TO EXTRANEOUS FACTORS

Perhaps a more common type of nonrandom measurement error, and certainly one that is more difficult to handle, stems from the influence of extraneous factors that may also be correlated with X and other variables in the causal system. We can think of numerous examples. When we deal with official data, such as that compiled by national censuses or government bodies, we are likely to find common biases affecting more than a single variable. For example a government may wish to appear more "advanced" economically and politically than it actually is. Size of population, gross national product, and degree of urbanization may all be inflated, whereas indices of crime and internal disruption may be biased in the opposite direction. Census underenumeration of Negroes may have varying effects on a number of different economic and political indicators.

In survey interviews or any other situation where attitudes, knowledge, or ability are being measured it is well recognized that motivational factors are likely to produce nonrandom errors. Respondents with low motivation or interest may tend to check items randomly. On ability or knowledge tests this is likely to lower their performance scores; on attitudinal items it may give them intermediate scores on many different scales. If the questions are tapping several underlying dimensions, without this being clearly recognized, there may be additional confounding effects. For example if many of the items permit responses that are conventional or popular, while others are more controversial, then persons who are conformists or "other directed" may respond differently from those who are nonconformists. This may hold true regardless of the subject being asked, so that there is a common component across several supposedly distinct scales.

Unfortunately, unless these extraneous factors are recognized and controlled in the data

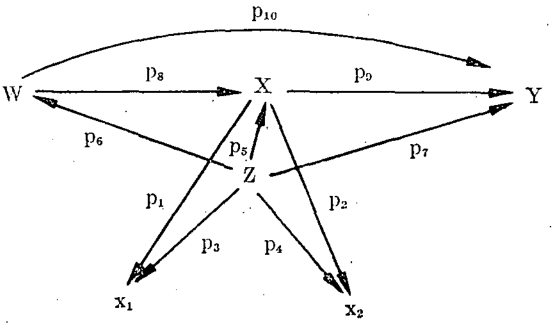

FIG. 7

collection stage they are likely to remain as unknowns. If they occur to an investigator as an afterthought, the chances are that they, themselves, will have been only indirectly measured. Furthermore, if there are numerous such extraneous factors operative it will be difficult to bring them all into the model in such a way that their relationships with the other variables are made explicit. Most commonly we consider such variables when we collect our data, and then we pretend that they no longer exist when we reach the analysis stage. Such a practice is no doubt necessary in order to preserve our scientific sanity. Nevertheless, it will not lead us to the correct conclusions unless the distorting effects are relatively minor.

Let us illustrate in terms of a relatively simple model involving X, which is again measured by two indicators; Y which is dependent on X and assumed perfectly measured (though similar results would be obtained if we had two indicators of Y with random errors or errors proportional to Y); W which is taken as a cause of both X and Y, and an extraneous factor Z taken as a cause of X_1 and X_2 and as possibly correlated with W, X, and Y. In the model of Figure 7, Z is assumed to cause W, X, and Y. Obviously there are many other possibilities, even confining our attention to the links between W, X, Y, and Z. All of the other possibilities I have examined, however, have led to the same basic conclusions. We may therefore use this particular model as illustrative of both the procedure that can be followed and the difficulties one can expect to encounter.

Treating X as unmeasured but the remaining variables as measured, we have five measured variables and therefore ten equations, one for each of the correlations that can be obtained from the data. There are also ten unknown path coefficients that can be found by solving these ten equations, which will be rather complex nonlinear equations that cannot be easily solved algebraically. Presumably, one could make use of iterative computer programs to obtain numerical solutions if all the data were available.

However, as previously indicated, Z will generally be either unknown or poorly measured. It is therefore important to examine the implications of the model assuming that Z is unknown and that one has available only the six intercorrelations among W, X_1, X_2, and Y. There are of course now four more unknowns than equations, so that additional simplifications will be necessary. Keep in mind that our objective is to estimate the causal links p_8, p_9, and p_{10} involving only W, X and Y. Therefore it is conceivable that some of these path coefficients can be estimated even though there are too many unknowns in the total system.

In introducing simplifications it is undesirable to place many restrictions on the relationships among W, X, and Y, other than linearity and one-way causation, since these are the variables of fundamental interest. Of course it is possible to assume away the links between Z and X_1 and X_2, but these are the very sources of nonrandom measurement error of concern to us. We might wish to consider a model in which Z affected X_1 but not X_2, which might be subject only to random measurement error. Unless there are two or more Xi unrelated to Z, however, this kind of simplification will not really help. Let us therefore consider the kinds of simplifications introduced if Z is unrelated to W, X, or Y.

It turns out that the greatest simplifications will occur if we set $p_6=0$, thereby taking out the link between Z and the independent variable W. In this case the six equations can be written down by inspection as follows:

$$r_{X_1X_2} = p_1p_2 + p_3p_4 + p_5(p_1p_4 + p_2p_3)$$

$$r_{X_1W} = p_1p_8$$

$$r_{X_2W} = p_2p_8$$

$$r_{X_1Y} = p_1(p_9 + p_5p_7 + p_8p_{10}) + p_3(p_7 + p_5p_9)$$

$$r_{X_2Y} = p_2(p_9 + p_5p_7 + p_8p_{10}) + p_4(p_7 + p_5p_9)$$
$$r_{WY} = p_8p_9 + p_{10}$$

The last of these equations contains only p_8, p_9, and p_{10} which are of major interest, and the second and third equations contain only p_1, p_2, and p_8. But the remaining equations contain too many compound paths to be of any help. If equivalent forms had been used in the measurement of X, so that it would be reasonable to assume that $p_1 = p_2$ and $p_3 = p_4$, the system would contain two redundant equations for testing purposes (equations 3 and 5), but there would still be too many unknowns for solution.

If we take out the link p_7 between Z and Y, while retaining p_5, we remove two terms each from the expressions for r_{X_1Y} and r_{X_2Y} but leave the remaining equations unchanged. Similarly, if we remove p_5 but not p_7 we achieve somewhat greater simplifications, but it still remains impossible to solve for p_3, p_9, and p_{10}. Let us therefore consider the case where we take out all of the links (p_5, p_6, and p_7) of Z with the variables of interest. Even with this very restrictive model the equations reduce to:

$$\begin{aligned} r_{X_1X_2} &= p_1p_2 + p_3p_4 & \qquad r_{X_1Y} &= p_1(p_9 + p_8p_{10}) \\ r_{X_1W} &= p_1p_8 & r_{X_2Y} &= p_2(p_9 + p_8p_{10}) \\ r_{X_2W} &= p_2p_8 & r_{WY} &= p_{10} + p_8p_9 \end{aligned}$$

We now have six equations and seven unknowns. But the equations are not all independent since the ratio p_1/p_2 may be obtained by dividing the second equation by the third or by dividing the fourth by the fifth. This gives us a redundant equation for testing purposes, but even in this very restricted model we still cannot solve for the path coefficients.

Finally, if we introduce the further restriction that $p_{10} = 0$, so that W does not affect Y except through X, the equations reduce to the set:

$$\begin{aligned} r_{X_1X_2} &= p_1p_2 + p_3p & \qquad r_{X_1Y} &= p_1p_9 \\ r_{X_1W} &= p_1p_8 & r_{X_2Y} &= p_2p_9 \\ r_{X_2W} &= p_2p_8 & r_{WY} &= p_8p_9 \end{aligned}$$

Although this set contains a redundant equation as before, and although the compound path p_3p_4 cannot be decomposed, it becomes possible to solve for the remaining paths. For example

$$p_8{}^2 = (p_8/p_9)(p_8p_9) = \frac{r_{X_1W}}{r_{X_1Y}}(r_{WY}) = \frac{r_{X_2W}}{r_{X_2Y}}(r_{WY})$$

and

$$p_9{}^2 = (p_9/p_8)(p_8p_9) = \frac{r_{X_1Y}}{r_{X\ W}}(r_{WY}) = \frac{r_{X_2Y}}{r_{X_2W}}(r_{WY})$$

Thus in order to estimate the path coefficients of interest we have had to impose a large number of restrictions on the model. In effect, we have had to assume that the source Z of nonrandom measurement error is uncorrelated with any of the remaining variables in the system. Not only this, but we have had to utilize a very simple model connecting W, X, and Y. If we were primarily interested in p_9 connecting X to its effect Y, we would be obliged, in this instance, to locate a cause W of X that does not affect Y by any other path. Recall that the method of instrumental variables required a similar kind of assumption. Although we have here examined only a single model, it may well turn out that the removal of nonrandom measurement errors of the type under consideration may require us to locate additional variables such as W that can be assumed to have very simple relationships with the variables of primary interest.

IV. CONCLUDING REMARKS

Although random errors and errors proportional to X present no formidable problems provided multiple indicators are available, we have seen that nonrandom errors produced by extraneous factors can be difficult to handle unless the causal situation is extremely simple or unless these extraneous factors can be measured and explicitly introduced. The last example illustrates an extremely important point. If we wish to allow for nonrandom errors in several variables, the models we develop will generally be complex and will contain a large proportion of unknowns relative to the number of estimating equations. Although one might wish to construct complex models for use in relating the variables of interest, along with complex models allowing for measurement error, simplicity will have to be introduced in order to solve for the unknowns.

As a general principle, it will be necessary to utilize very simple causal models connecting the variables of real interest *if* one allows for complex models involving measurement errors. Conversely, if one wishes to test and estimate parameters in complex causal models, he will need to keep his asumptions regarding measurement error extremely simple. It would be nice to have it both ways, but this does not seem possible. The implication, of course, is that if we are to test complex models we must purify our measures so as to enable us to keep measurement error assumptions as simple as possible.

Obviously we need to give much more emphasis to measurement error problems than we have done thus far. This will require a higher degree of coordination to standardize measures so that results of different studies can be meaningfully compared and knowledge about measurement errors accumulated. Many of our global concepts, such as political alienation, normative consensus, democracy, and the like, are highly abstract and far removed from the operational level. Therefore, each may require indicators that are several steps removed. Whether or not such concepts can be used in models that are even indirectly testable remains to be seen.

Certainly we need to take a careful look at the various mechanisms that produce nonrandom measurement errors. We have considered only a few simple cases. But there may also be errors produced by nonadditive relationships with confounding variables, or nonlinear relationships between indicators and the underlying variables. Whenever a model implies reciprocal causation, so that least squares is no longer appropriate, it may be much more difficult to handle even random errors of measurement. Finally, as noted above, we have not yet seriously studied possible complications introduced in the aggregation process.

Although it may prove discouraging to note the existence of such complications for our analyses, progress can only be achieved by identifying problems one by one, formulating them as clearly as possible, and then studying the conditions under which each can or cannot be resolved.

MODERNIZATION AND THE POLITICS OF COMMUNALISM: A THEORETICAL PERSPECTVE[1]

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It has been said that technological and economic development lead ultimately to the decline of communal conflict, and that the emergence of new kinds of socio-economic roles and identities undercuts the organizational bases upon which communal (that is, "racial," "ethnic," "religious," or "tribal") politics rests. In the past decade, several scholars working in culturally plural societies have challenged this conventional view. They have suggested that communalism may in fact be a persistent feature of social change, and that the dichotomous tradition-modernity models which have often guided our empirical investigations have obscured this theoretical alternative and thereby produced false expectations concerning the direction of change.[2] This paper attempts to synthesize the various elements of this emerging theoretical perspective through the formulation of several propositions which link modernization to communalism. While our discussion will draw primarily upon the Nigerian experience for illustrative material, the propositions are intended to be applicable across societies.

"Communalism," in this paper, refers to the political assertiveness of groups which have three distinguishing characteristics: first, their membership is comprised of persons who share in a common culture and identity and, to use Karl Deutsch's term, a "complementarity of communication;"[3] second, they encompass the full range of demographic (age and sex) divisions within the wider society and provide "for a network of groups and institutions extending throughout the individual's entire life cycle;"[4] and, third, like the wider society in which they exist, they tend to be differentiated by wealth, status, and power. Communal demands and conflict are politically distinctive in that they may reflect a desire for separation and may threaten to alter the political boundaries of the wider society.[5] The Nigerian experience, we will contend, runs counter to the view that communal "particularism" or "communalism" is an historical anachronism, ultimately destined to be submerged by the "universalistic" tidal wave of modernity. An analysis of the Nigerian case suggests, rather, that modernization, far from destroying communalism, in time both reinforces communal conflict and creates the conditions for the formation of entirely new communal groups.

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[1] Research was supported by the Departments of Political Science at Michigan State University and Western Michigan University, and by the African Studies Center at Michigan State University. We both greatly benefitted by association with faculty and students in a workshop on the Nigerian experience sponsored by the Midwest Universities Consortium for International Activity. We also wish to acknowledge, with gratitude, the helpful comments of the following colleagues who read this paper in manuscript form: Karl W. Deutsch, S. N. Eisenstadt, Justin Green, Daniel Lerner, James O'Connell, Simon Ottenberg, Lucian W. Pye, Lloyd I. and Susanne Hoeber Rudolph, Richard L. Sklar, Saadia Touval and Aristide Zolberg. An earlier version of this paper was presented at the meetings of the American Political Science Association in New York, September 1969.

[2] In particular, see the following works: Charles W. Anderson, Fred R. von der Mehden, and Crawford Young, *Issues of Political Development* (Englewood Cliffs: Prentice-Hall, Inc., 1967) pp. 15–83; James S. Coleman, *Nigeria: Background to Nationalism* (Berkeley and Los Angeles: University of California Press, 1960); S. N. Eisenstadt, "Reflections on a Theory of Modernization," in Arnold Rivkin (ed.), *Nations by Design* (Garden City: Doubleday and Co., 1968), pp. 35–61; Clifford Geertz, "The Integrative Revolution," in Clifford Geertz (ed.), *Old Societies and New States* (Glencoe: The Free Press, 1963), pp. 105–157; Martin Kilson, *Political Change in a West African State: A Survey of the Modernization Process in Sierra Leone* (Cambridge: Harvard University Press, 1966); Lloyd I. and Susanne Hoeber Rudolph, *The Modernity of Tradition: Political Development in India* (Chicago: University of Chicago Press, 1967); Richard L. Sklar, "The Contribution of Tribalism to Nationalism in Western Nigeria," *Journal of Human Relations*, 8 (Spring-Summer 1960), 407–415; Myron Weiner, *The Politics of Scarcity: Public Pressure and Political Response in India* (Chicago: University of Chicago Press, 1962); C. S. Whitaker "A Dysrhythmic Process of Political Change," *World Politics*, 19 (January 1967), 190–217; W. Howard Wriggins, "Impediments to Unity in New Nations: The Case of Ceylon," this REVIEW, 55 (June 1961), 313–320; Crawford Young, *Politics in the Congo* (Princeton: Princeton University Press, 1965); Aristide Zolberg, *One-Party Government in the Ivory Coast* (Princeton: Princeton University Press, 1964).

[3] According to Deutch, complementarity of communication in a group "consists in the ability to communicate more effectively and over a wider range of subjects with members of one large group than with outsiders." See his pathbreaking *Nationalism and Social Communication* (Cambridge: M.I.T. Press, 1953), p. 71.

[4] Milton Gordon, *Assimilation in American Life* (New York: Oxford University Press, 1964), p. 39.

In these respects, the Nigerian case is by no means unique. The Nagas have not become Indians indistinguishable from other Indians, any more than Ibos and Yorubas and Tivs have become interchangeable Nigerians. Overseas Chinese and Javanese have not become Indonesians. Nor have French Canadians and Black Americans "melted" into their respective wider societies. In each of these cases, the contemporary pattern of communal formation and conflict may be seen as an immediate by-product of the modernization process.

The Nigerian case is a particularly appropriate illustration of the relationship between communal conflict and modernization in that, on most social and economic indices, Nigeria showed signs of relatively rapid growth in comparison to other developing nations subject to comparable ecological and historical conditions.[6] Accompanying these developments were a high rate of social mobilization and a significant expansion of the traditional frameworks of empathy and social identification. It was precisely Nigeria's success in these economic and social spheres which produced the constantly optimistic appraisals of the course of Nigerian development. But economic and social success were not matched in the political sphere and, in retrospect, it is clear that Nigeria's very economic and social progress sowed the seeds of the nation's political crisis. That crisis, it will be argued, is traceable neither to poverty nor, as some popular accounts would have it, to the simple fact of cultural diversity and the reassertion of traditional tribal antagonisms. On the one hand, the group frustrations which underlie communal antagonisms are as much a reflection of change and the blocking of new aspirations as they are of impoverishment. On the other hand, inter-group conflict is seldom a product of simple cultural diversity and, in the Nigerian case, there is little that is "traditional" about the contemporary pattern of political divisions. On the contrary, Nigeria's political crisis is traceable directly to the widening of social horizons and to the process of modernization at work within the national boundaries.

Social Mobilization and Communal Conflict

The propositions discussed in this paper attempt to unravel the complicated web of factors which link communalism with modernization. The fourteen propositions listed below focus upon four themes: (1) competitive communalism; (2) institutional communalism; (3) communal transformation; and (4) communal compartmentalization. Following their listing, we turn to a more detailed analysis of each of these themes.

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[5] It should be noted that communal groups are not alone in sharing culture, identity and complementarity. An age-grade among the Masai, for example, does in fact share in these characteristics. So does the upper class in Britain. But age grades and social classes are not to be thought of as communal groups, because they do not encompass the full range of demographic divisions within a society and they are not socio-economically heterogenous. It is these latter characteristics which give to communal groups their distinctive political significance in that they are, to use Clifford Geertz' term, "candidates for nationhood." See Geertz, *op. cit.*, p. 111. The term, "Communalism," is not to be confused with the concept of "communal participation" developed by Richard L. Sklar, in his *Nigerian Political Parties: Power in an Emergent African Nation.* (Princeton: Princeton University Press, 1963), pp. 474–480.

[6] Carl K. Eicher, for example, has written, "Nigeria's rate of economic growth of approximately 5 percent per annum since 1960 is impressive and second only to that of the Ivory Coast among West African nations. Nigeria became the world's tenth largest exporter of petroleum in 1966 and its petroleum reserves are estimated to match those of Libya." See his "The Dynamics of Long-Term Agricultural Development in Nigeria," *Journal of Farm Economics*, 49 (December 1967), 1158–1170. According to an estimate of Carl Liedholm, Eastern Nigeria in the early sixties possessed the fastest growing industrial sector in the world. See Carl Liedholm, "Preliminary Estimates of an Index of Industrial Production for Eastern Nigeria, from 1961–66," (Enugu: Economic Development Institute, March 1967). For a more extensive discussion of Nigerian economic development, see Gerald K. Helleiner, *Peasant Agriculture, Government, and Economic Growth in Nigeria* (Homewood: Richard Irwin, Inc., 1966). For lucid discussions of the political consequences of Nigeria's rapid rates of educational expansion and urbanization, see James S. Coleman, *op. cit.* and David Abernethy, *The Political Dilemma of Popular Education: An African Case* (Stanford: Stanford University Press, 1969).

I. *Competitive Communalism*

1. In a culturally plural society, the competition engendered by social mobilization will tend to be defined in communal terms.
2. Differential rates of mobilization among communal groups exacerbate communal conflict by multiplying coincident social cleavages.
3. In the short term, the reduction of communal imbalances in wealth, status or power may serve to intensify communal antagonisms.

II. *Institutional Communalism*

4. Social or geographical segregation of communal groups may lessen communal conflict if it reflects a mutally agreed upon reduction or elimination of competitive communal interaction; however, if mobilized and competitive communal groups desire, or are forced, to interact within the same system of social stratification, segregation may intensify communal conflict.
5. The formation of communal associations, in response to inter-communal contact and competition, furthers communal conflict.
6. The subordination of political institutions to the interests of particular communal groups tends to reinforce and politicize communal conflict.
7. The greater the number of equally powerful communal groups, the greater the likelihood that political institutions will be able to retain their coherence and impartiality vis-a-vis communal competitors.
8. Political institutions which encourage the participation of the masses in the recruitment of leaders tend to further politicize and intensify communal conflict.

III. *Communal Transformation*

9. In an environment of social mobilization and communal competition, communal groups will tend to fuse or to expand their traditional boundaries to include groups and individuals with whom they can identify and who might prove useful allies during inter-group conflict.
10. Mobilization and competition lead to the increasing internal differentiation of communal categories along socio-economic and ideological lines.
11. Accompanying communal fusion and expansion is the deparochialization of traditional group ties and perspectives.
12. Communal transformation entails the multiplication rather than the substitution of social identities.

IV. *Communal Compartmentalization*

13. Since politically salient identities are situationally specific, communal and non-communal political orientations may well coexist within the same person.
14. Since communal and non-communal political orientation may coexist within individuals, each activated in different social contexts, modernization need not eliminate communal differentiation and conflict.

I. COMPETITIVE COMMUNALISM

Proposition 1. In a culturally plural society, the competition engendered by social mobilization will tend to be defined in communal terms.

A convenient starting point of this analysis of modernization and communal conflict is the concept of "social mobilization," defined by Karl Deutsch as "the process in which major clusters of old social, economic and psychological commitments are eroded or broken and people become available for new patterns of socialization and behavior."[7] Social mobilization, in our view, generates the new kinds of social competition and new kinds of scarcity which underlie contemporary communal antagonisms.

The linked processes of change subsumed within the concept of social mobilization—such as urbanization, the introduction of mass education, the diffusion of the mass media, the development of increasingly efficient and productive commercial networks—alter the human landscape in two significant respects. First, "social mobilization" means the reorientation of a large number of citizens to a new system of rewards and paths to rewards in all spheres of society. People's aspirations and expectations change as they are mobilized into the modernizing economy and polity. They come to want, and to demand, more—more goods, more recognition, more power. Significantly, too, the orientation of the mobilized to a common set of rewards and paths to rewards means, in effect, that many people come to desire precisely the same things. Men enter into conflict not because they are different but because they are essentially the same. It is by making men "more alike," in the sense of possessing the same wants, that modernization tends to promote conflict.

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[7] Karl W. Deutsch, "Social Mobilization and Political Development," this REVIEW 55 (September 1961), 493–514; reprinted in Jason L. Finkle and Richard W. Gable (eds.), *Political Development and Social Change* (New York: John Wiley and Sons, Inc.) pp. 205–226.

Second, "social mobilization" means not only the emergence of a new set of motivating values and career paths, but also that the demand for scarce resources cannot keep up with their supply. Nowhere is the reality of "modern scarcity" experienced more intensely than in the cities, wherein the rate of population growth almost invariably exceeds the rate of economic development and the availability of new jobs. It is here that the various elements of the mobilized population are thrown into direct, and very personal, competition with one another—for positions within governmental agencies and commercial concerns, for the control of local markets, for admission to crowded schools, for induction into the army, and for control of political parties. It is not surprising, therefore, that many of the competitors perceive themselves as involved in a "zero-sum game," in which one man's failure is attributable to another man's success. While this perception may or may not be accurate in any given instance, the perception itself leads to the increasing competitiveness of the modern sector.

It is against this backdrop of social mobilization and a highly competitive modern sector that communal conflict in culturally plural societies must be understood.[8] On the one hand, it is the competitor within the modern sphere who feels the insecurities of change most strongly and who seeks the communal shelter of "tribalism."[9] The tradition-bound rural villager, though he may indeed be caught up into the competitive struggle of his urban-based kinsman, by contrast experiences contemporary tribalism only vicariously and at second-hand. On the other hand, men become tribalists not only out of insecurity but also out of the many opportunities created by social mobilization in a communal milieu. In culturally plural societies, citizens tend to perceive their competitive world through a communal prism and to be responsive to communal appeals. Communalism therefore becomes a matter of opportunism. It matters not that, in any given competition, communal criteria are inappropriate to the determination of the outcome and may not in fact have been operative. What is important is that the personal fortunes of individuals are generally believed to depend on their communal origins and connections. This being the case, individuals plan and organize accordingly. Thus, the aspirant Nigerian politician seeks to mobilize his "tribal union" behind his candidacy; at the same time, his towns-people—those resident in their home community as well as those resident in the alien city—view his candidacy as an expression of their group aspirations and his election as an indicator of group recognition and power. (Conversely, the members of other communal groups view his candidacy as a threat to their own group aspirations and vested interests.) Similarly, the job applicant or prospective schoolboy turns to his kinsmen for assistance, while his benefactors view the job or financial assitance they extend as a communal investment to be reciprocated by the beneficiary. In short, the common expectation of the primacy of communal criteria produces the self-fulfilling prophecy of communally-oriented competitive strategies in virtually all walks of life.

Proposition 2. Differential rates of mobilization among communal groups exacerbate communal conflict by multiplying coincident social cleavages.

The various communal groups which comprise a culturally plural nation-state are seldom mobilized at the same rate, with the consequence that the members of the more slowly mobilized communities are placed at a disadvantage in the competition for the national rewards of wealth, status and power. This section explores, first, the factors underlying differential mobilization and, second, the implications of differential mobilization for the character of communal conflict.

There are at least three analytically distinguishable factors which work to create intergroup differentials in the developmental process—the nature of Western contact, environmental opportunities and cultural predispositions. While it is difficult to assign weights denoting the relative significance of each, it is apparent that all are involved in determining the timing and pace of social mobilization. First, variations in the timing, intensity and character of Western contact yield group variations in the rate of urbanization, the spread of literacy, the diffusion of the mass media, the acculturation of Western norms and techniques. In Nigeria, for example, accidents of history and geographical location have been especially important in producing very different kinds of contact experiences and resultant rates of social change as between North and South. Thus, the exclusion of missionaries and hence Western schools from the Islamic north was a crucial factor accounting for the subsequent domination by Southerners of both administrative and technical occupations. Similarly, coastal and riverain communities, which were relatively more accessible to the colonizing agents, tended to have contact with the West at an earlier date than communities situated in rural hinterlands, and tended often to be subject to a relatively more intensive and qualitatively quite different acculturative experience.

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[8] Following Anderson, von der Mehden and Young, "cultural pluralism" refers to "the existence within a state of solidarity patterns, based upon shared religion, language, ethnic identity, race, caste, or region, which command a loyalty rivaling, at least in some situations, that which the state itself is able to generate," *op. cit.*, p. 17.

[9] For an insightful discussion of the psychological dimension of social change, see Lucian W. Pye, *Politics, Personality and Nation-Building* (New Haven: Yale University Press, 1962).

Second, variations in the kinds of environmental opportunities available to different communities have also contributed to variations in the rates of social mobilization. Such variables as the availability of mineral resources, the fertility of the soils, the export value of indigenous agricultural commodities, the proximity of a transportation crossroads—all condition the pace of social change and the economic potentialities of a given community. Just as Western Nigerian economic growth in the post-war period was at least partially attributable to the profitability of the indigenous cash crop of cocoa, so Northern Nigeria's relative poverty and lower developmental rates were partially attributable to the harsh economic reality that groundnuts were a less valuable agricultural commodity.[10] More recently, the discovery of significant petroleum deposits in Eastern and Mid-Western Nigeria has meant that the developmental potential of this sector has become very large indeed. Yet another example of environmental influences on social mobilization is that of land shortage in densely populated Iboland contributing to the high out-migration of Ibos to Nigeria's urban centers. As a result of this out-migration, Ibos came to constitute the principal stranger-populations of Northern and Western cities, with many individual Ibos performing highly visible commercial, administrative and technical functions.

Finally, variations in the cultural predispositions of different groups produce variations in their response to Western contact and to existing environmental opportunities. As the work of McClelland, Hagen, LeVine, Young, and Ottenberg suggests, some cultural groups appear to be more predisposed than others to compete successfully for the rewards of modernization.[11] Thus, in the Nigerian case, it has been suggested that relatively rapid rates of mobilization and development in Eastern Nigeria are traceable, in part, to the receptivity to change on the part of the Ibos and to the relatively high incidence of "need achievers" to be found within this cultural group.[12]

The consequence of communal groups being mobilized at different rates is that some peoples gain a head-start in the competition for the scarce rewards of modernity. New socio-economic categories therefore tend to coincide with, rather than intersect, communal boundaries, with the consequence that the modern status system comes to be organized along communal lines. There is, consequently, little to ameliorate the intensity of communal confrontations. The fewer the cross-cutting socio-economic linkages, the more naked such confrontations and the greater the likelihood of secessionist and other movements of communal nationalism.

Thus, in the Nigerian case, a Northern Hausa —because of differential mobilization—came to be not only culturally dissimilar from an Eastern Ibo; he came also to be distinguished by his Islamic religion (as contrasted with the Christianity of the Ibo), his relative poverty, and his relative educational backwardness. In short, the lines of cleavage as between Hausa and Ibo all coincided. As a result, conflict of any sort—whether over jobs or markets or political office —threatened an all-out communal struggle.

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[10] Concerning the groundnut industry, see J. S. Hoggendorn, "The Origins of the Groundnut Trade in Northern Nigeria," in Carl Eicher and Carl Liedholm (eds.), *Growth and Development of the Nigerian Economy,* (East Lansing: Michigan State University Press, 1970), pp. 30–52.

[11] See David C. McClelland, *The Achieving Society* (Princeton: Van Nostrand, 1961); Everett E. Hagen, *On the Theory of Social Change* (Homewood: Dorsey Press, 1962); Crawford Young, *op. cit.,* especially pp. 256–265; Robert A. Le Vine, *Dreams and Deeds: Achievement Motivation in Nigeria* (Chicago and London: The University of Chicago Press, 1965); Simon Ottenberg, "Ibo Receptivity to change," in William R. Bascom and Melville J. Herskovits (eds.), *Continuity and Change in African Cultures* (Chicago and London: The University of Chicago Press, 1962), pp. 130–143.

[12] The precise role and significance of such cultural predispositions in the development process is as yet uncertain. For a criticism of the McClelland thesis, see S. N. Eisenstadt, "The Need for Achievement," *Economic Development and Cultural Change,* 11 (January 1963), 420–431. For a critical analysis of the Ibo "receptivity to change" hypothesis, see Richard Henderson, "'Generalized Cultures' and 'Evolutionary Adaptability': The Comparison of Urban Efik and Ibo in Nigeria," *Ethnology,* 5 (October 1966), 365–391.

Differential mobilization in Nigeria widened the socio-political cleavages not only between the country's major nationality groups but also between majority and minority communal groups within each administrative region and even between sub-groups of a single nationality. Thus, differential rates of educational expansion and economic growth in Eastern Nigeria heightened the previously latent cleavage between the landlocked Owerri Ibo interior, on the one hand, and the more advantaged marginal Ibo (Onitsha, Oguta, Aro) riverain groups and non-Ibo (Ijaw, Ibibio, Efik) coastal communities, on the other.[13] In similar fashion, the commercially prosperous Lagos and Ijebu Yoruba came to be set off from the less advantaged Yoruba interior. Likewise, within Northern Nigeria, the non-Hausa speaking Middle Belt developed economically and educationally more rapidly than the upper North; and, within the upper North, those Emirates which played a significant role in the colonial administrative and commercial systems—such as Kano and Katsina—assumed preeminent positions in the modern stratification system.[14] In short, differential mobilization had the effect of creating sets of coinciding cleavages within as well as between the three dominant cultural core-areas.[15]

Proposition 3. In the short term, the reduction of communal imbalances in wealth, status or power may serve to intensify communal antagonisms.

While it seems clear that, given an environment of social mobilization, communal imbalances in wealth, status and power exacerbate communal tensions, it does not follow that progress toward the elimination of such imbalances will be accompanied by communal accommodation. It is true that inter-group equalization is a necessary condition for the emergence of cross-cutting socio-economic linkages. That does not mean, however, that the members of different communal groups will in fact perceive the commonality of their socio-economic interests or that, if they do perceive this commonality, that they will act upon it. In the short term, at least, there are two factors which may link progress toward inter-group equalization with the intensification rather than the amelioration of communal antagonisms.

First, upward communal mobility normally leads to the alteration of reference-groups on the part of the communal participants and, hence, to the alteration of their perceptions of their relative status vis-a-vis their communal competitors.[16] This is what appears to have been involved in the determination of the late-starting Ibos to "catch-up" with—indeed, surpass—Western Nigeria's Yorubas, a group with which the Ibos did not even interact in the pre-colonial period. In a very real sense, it is their equality rather than their inequality which is at the heart of contemporary Ibo/Yoruba communal conflict. In short, communal mobility, rather than producing a sense of competitve gain and satisfaction, may lead to a deepening sense of relative deprivation and communal insecurity. The actual intensity of these feelings of communal deprivation and insecurity will depend very largely upon the responsiveness of the wider society to the new communal assertiveness. The blocking of new communal aspirations to wealth, status and power will tend to intensify group frustrations and radicalize political response.

Second, communal mobility may intensify communal dissatisfaction by producing increased impatience with the remaining imbalances. The closer the members of a formerly deprived group approach equality on one of the dimensions of wealth, status and power, the more intolerable becomes inequality on the other two dimensions. It is to this phenomenon that the "revolution of rising aspirations and frustrations" refers. This notion seems to fit more closely the American race experience, for example, than the simple observation that black deprivation leads to black militancy. It is seldom the very poorest who are the most militant.[17]

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[13] See Howard Wolpe, "Port Harcourt: Ibo Politics in Microcosm," *Journal of Modern African Studies*, 7 (October 1969), 469–493.

[14] For an excellent analysis of communal conflict and violence within Nigeria's upper north, see John Paden, "Communal Competition, Conflict, and Violence in Kano," to appear in a forthcoming volume edited by Robert Melson and Howard Wolpe, *Nigeria: Modernization and the Politics of Communalism* (East Lansing: Michigan State University Press, 1971).

[15] For a parallel analysis of Ivory Coast inter-group relationships, see Aristide R. Zolberg, *op. cit.*

[16] Daniel Lerner, for example, has suggested that "A person with high achievement may still be dissatisfied if his aspirations far exceed his accomplishments. Relative deprivation . . . is the effective measure of satisfaction among individuals and groups." See his "Toward a Communication Theory of Modernization," in Lucian W. Pye (ed.), *Communications and Political Development* (Princeton: Princeton University Press, 1963), p. 333. For a parallel treatment of the problem of relative deprivation, see Ulf Himmelstrand's application of rank-equilibration theory to Yoruba-Ibo competition in "Tribalism and Nationalism in Nigeria," in Melson and Wolpe, *op. cit.*

II. INSTITUTIONAL COMMUNALISM

To this point in our discussion, we have considered the relationship between social mobilization and communal conflict without reference to the institutional context in which these processes unfold. But this context is itself directly related to the nature and intensity of communal conflict. Institutions which divide groups from each other *at the same time as they encourage political competition and participation* can only exacerbate communal conflict. Of course, these separatist institutions (such as residential segregation and communal associations) are themselves a reflection of existing patterns of communal conflict. This reciprocity between institution and conflict comprises the subject matter of the next set of propositions.

Proposition 4. Social or geographical segregation of communal groups may lessen communal conflict if it reflects a mutually agreed upon reduction or elimination of competitive communal interaction; however, if mobilized and competitive communal groups desire, or are forced, to interact within the same system of social stratification, segregation may intensify communal conflict.

Some scholars have been tempted to conclude that social mobilization and, in particular, urbanization, entailing as they normally do complex functional inter-relationships between culturally diverse populations, would significantly contribute to the destruction of communal barriers and the creation of integrated and socially cohesive national communities. This view closely parallels the popular "melting pot" hypothesis of American folklore and sociology which asserts that inter-group contact in the United States has led to the "fusion" of these groups so as to produce a distinctive American national stock.

Such a view—whether applied to the developing African continent or to the economically prosperous United States—lacks secure empirical foundation. In the United States, for example, inter-group interaction, while producing a fairly high degree of *cultural* assimilation, has produced far less *structural* assimilation than is conventionally assumed. While most white Americans have been assimilated within the secondary institutions of the society, their primary group relationships remain highly segregated by nationality or religion, and ethnicity remains a persistent fact of political life. Insofar as race relations are concerned, structural assimilation has been almost non-existent—Blacks merely abandoning the segregation of the rural South for the segregation of the urban North—and in some cases even the trend toward cultural assimilation seems to have been reversed. In short, most Americans, despite their adherence to a common set of national norms, live out their private lives in fairly tight and often segregated communal compartments.[18]

Much the same may be said of the citizens of other culturally plural nation-states. In Nigeria, for example, while isolated instances of cultural and/or structural assimilation may be cited, most urban communities evolved segregated residential patterns, especially with regard to the "strangers" vis-a-vis the indigenous host population. The isolation of Ibos in the *Sabon Garis* of Northern Nigeria is only the most dramatic example of this pattern of urban segregation. Segregated Hausa quarters were similarly to be found in the major towns of Eastern and Western Nigeria, and urban life was invariably compartmentalized according to communal categories.[19]

The relationship between communal segregation and communal conflict is more complicated than conventionally assumed. Segregation per se does not create or intensify communal conflict. On the contrary, to the extent that communal segregation reflects a mutually agreed upon reduction or elimination of competitive communal interaction, segregation means the reduction or elimination of sources of potential communal conflict. Indeed, history yields several examples of segregated communities living in peace. Whether in the *ante bellum* southern United States, in the religiously pluralistic Ottoman Empire, or in Nigeria's feudal North, segregation maintained inter-group peace while preserving mutually accepted notions of group privilege and status. Similarly, there are examples in which the separation of groups in conflict has lead—as a result of a decline in competitive interaction—to a decline in the level of communal conflict. This would appear to be one result, for example, of the fragmentation of the Austro-Hungarian Empire and of the partition of India into Moslem and Hindu states.

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[17] For an extensive discussion of how "deprivation" enters into civil strife, see Ted Gurr, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," this REVIEW, 62 (December 1968), 1104–1124.

[18] The terminology is Milton Gordon's. See *op. cit.*, pp. 60–83. "Cultural assimilation" implies that a group has changed its cultural patterns to those of the host society. "Structural assimilation" refers to the group's membership actually entering into the cliques, clubs, and institutions of the host society, on the primary group level.

[19] For an incisive analysis of Yoruba/Hausa segregation and the communalization of commercial activity in the city of Ibadan, see Abner Cohen, "The Social Organization of Credit in a West African Cattle Market," *Africa*, 35 (1965), 8–19. For related discussions of inter-communal trade, see E. Wayne Nafziger, "Inter-Regional Economic Relations in the Nigerian Footwear Industry," *The Journal of Modern African Studies*, 6 (1968), 531–542, and John Harris and Mary Rowe, "Entrepreneurial Attitudes and National Integration," Melson and Wolpe, *op. cit.* See also the forthcoming studies by Barbara Callaway on Aba urban politics and by George Jenkins and K. W. S. Post on the political history of the important Ibadan political leader, the late Adelabu.

If, however, mobilized and competitive communal groups desire, or are forced, to interact within the same system of social stratification—as in the contemporary United States—segregation may intensify communal conflict. First, under these circumstances the effect of communal segregation is to preserve communal inequalities in wealth, status and power. In the abstract, segregation need not mean inequality; it can be, rather, a means of allowing communities a modicum of autonomy in their cultural and social life. In the modern world, however, segregation has meant more than just cultural and social autonomy; it has meant also unequal access to education, to the economy, and to political power. It is by maintaining and promoting such inequalities that segregation has promoted communal conflict.

Second, in a competitive political system (see Proposition 9), the social and physical separation of communal groups encourages the development of communally-based political institutions and strategies. Thus, it is the geographical concentration—and separation—of Black Americans in urban areas which makes the drive for urban "Black power" politically viable. It may be that, in the long run, "Black power," which is in part a product of segregation, will be able to achieve a breakthrough in the relative wealth, status, and power of Blacks. But it should be noted that segregation also permits the political and economic isolation of the Black minority by the dominant white community, an isolation which might well have the effect of preserving communal inequalities.

Third, the geographical separation of communal groups means that territorial or residential interests come to coincide with communal interests. Consequently, the range of political issues which activate communal identities and hence communal conflict is widened. Thus, the siting of an industry or the allocation of social amenities (e.g., paved roads, medical centers, schools) may become the occasion for communal confrontation. This, in effect, is but a corollary of the proposition that coincident social cleavages operate to exacerbate communal conflict (Proposition 2).

Fourth, as Gordon Allport has observed, "Segregation markedly enhances the visibility of a group; it makes it seem larger and more menacing than it is."[20] So it was in the case of the Ibo strangers resident in Northern Nigeria. The conspicuousness Ibos possessed by virtue of their economic, technical and administrative pre-eminence was enlarged by their physical separation from the indigenous community. This segregation was partly self-imposed and partly the result of Northern exclusionary policies. Laws were passed, for example, specifically forbidding non-indigenes from acquiring local land. In some instances restrictions were placed upon Ibo commercial activity. The important point, however, is that whatever the causes of Ibo segregation might have been, their physical separation from the host community furthered the out-group stereotype of the Ibos as "clannish" and "concerned only with their own." In short, communal segregation was an essential ingredient of urban tensions and violence in Northern Nigeria.

Proposition 5. The formation of communal associations in response to inter-communal contact and competition tends to further communal conflict.

Throughout the developing world, scholars have noted the rise of what have come to be known as "communal associations" or "para-communities" in Asia, "improvement associations" or "tribal unions" in Africa. Such unions, associations, and para-communities are a blend of both the cosmopolitan and the parochial. In much the same manner as the immigrant political machines of America's urban communities, the communal organizations of Asia and Africa provide certain badly needed social services. They tend to be future oriented, and concerned with achievement and social welfare not only in the cities, where most of their members live, but also in the rural hinterland, which remains an important reference point for many urban dwellers. By helping to promote loyalty to communal groups, communal associations reduce the discontinuities inherent in migration and rapid social change.[21]

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[20] Gordon W. Allport, *The Nature of Prejudice* (Garden City: Doubleday and Company, Inc., 1958), p. 256.

[21] See for example, Kenneth Little, *West African Urbanization: A Study of Voluntary Associations in Social Change* (Cambridge: Cambridge Univer-

The important point, however, is that as community associations service their respective constituents, increasingly large numbers of urban residents develop a vested interest in their communal identities and organizational affiliations. Moreover, it has become clear that some communal groups have more of what it takes to organize than do others, with the result that whole occupational categories come to be monopolized by particular associations and their members. For these reasons, communal associations tend to stoke the engine of differential social mobilization and its attendant communal conflict.

The examples of communal organization in Nigeria are legion. The Ibos and Ibibios of Eastern Nigeria assumed the initial organizational initiative during the 1920's and 1930's, and their success inspired similar initiatives among Nigeria's other communal groups. Although in their inception these oganizations served mainly social and economic functions, they soon became politicized, acting as communal pressure groups both with regard to the party and to the government. Thus, local improvement associations would attempt to influence the selection of party nominees to stand election in local electoral constituencies or would make demands upon government officials with respect to the allocation of amenities to their home communities. Similarly, at higher levels of cummunal structure, such organizations as the Ibo State Union and the Yoruba-speaking Sons of the Descendants of Oduduwa furthered the development of nationality-wide communal consciousness, and advanced the interests of their respective nationalities within party and governmental circles.[22] Significantly, however, as communally-oriented single-party systems became established within each region, and national politics became a matter of competition between the three communally-based regional parties, the nationality-wide organizations found their political functions usurped. The parties, in effect, became the defenders of majority communal interests. However, at the sub-nationality level of town and clan, "improvement unions" continued to play a significant role in intra-party political conflict.[23]

Proposition 6. The subordination of political institutions to the interests of particular communal groups tends to reinforce and politicize communal conflict.

The degree of intensity of communal conflict will be determined very largely by what Huntington has termed the "autonomy" of the society's political institutions. To quote Huntington,

> Political institutionalization, in the sense of autonomy, means the development of political organizations and procedures that are not simply expressions of the interests of particular social groups. A political organization that is the instrument of a social group—family, clan, class—lacks autonomy and institutionalization.[24]

If political institutions do not possess institutional integrity and appear to be in the control of particular communal interests, those communal groups lacking power and position will tend to question the legitimacy of the institutional order and will be encouraged politically to "go it alone." It should be noted that it is not the nature of primary institutions and primary group interaction which are at issue; people may in fact live their private lives within their communal compartments without jeopardizing the political viability or stability of the wider society. What *is* at issue is the character of those secondary institutions of the society through which the members of communally discrete subsocieties may relate to one another in a systematic and orderly fashion.

This analysis may be illustrated by reference to both the American and the Nigerian cases. Thus, in the United States, communal separatism at the primary group level—that is, with respect to marriage, friendship cliques, etc.—has characterized intra-white as well as Black/white relationships. However, in the case of white Americans, the secondary institutions of the national society were sufficiently flexible and autonomous as to be relevant and legitimate for virtually all communal elements. Only in the case of non-whites—most notably, of course, of Black Americans—were the secondary institu-

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sity Press, 1963); Immanuel Wallerstein, "Ethnicity and National Integration in West Africa," in Pierre L. Van Den Berghe (ed.), *Africa: Social Problems of Change and Conflict* (San Francisco: Chandler Publishing Company, 1965) pp. 472–482. For careful analyses of this phenomenon in India see Lloyd I. and Susanne Hoeber Rudolph, *op. cit.*, and Myron Weiner, *op. cit.*

[22] Oduduwa is a culture hero and mythical progenitor of the Yoruba people. See Sklar, *op. cit.*, p. 67.

[23] Interesting discussions of the relationship between improvement unions and political parties are to be found in Audrey Smock, "The Political Role of Ibo Ethnic Unions" and Alvin Magid, "The Idoma State Union: Minority Politics in Northern Nigeria," in Melson and Wolpe, *op. cit.*

[24] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 20.

tions unresponsive and exclusionary. In effect, from the perspective of the wider society inclusive of both Blacks and whites, the political institutions of the society have never possessed autonomy but, rather, have been historically subordinated to the interests of the majority racial group. It is not surprising therefore that the most determined separatist sentiment in the United States has been voiced by the members of that group which has been least participant—in the sense of sharing in responsible decision-making power—in the secondary institutions of the society.[25]

Similarly, in Nigeria both national and regional political institutions came to be identified with the interests of particular communal groups, thereby impairing their legitimacy in the eyes of minority communal interests at both the national and the regional levels.[26] More concretely, with the creation of Nigeria's Federal constitutional system and, in particular, with the creation of regional sources of wealth and power, it became clear that political parties which controlled the machinery of government could control the political and economic fate of the region.[27] Since Nigeria's administrative boundaries were so drawn as to give clear numerical superiority to a single nationality within each region, political parties on appealing to majority communal interests quickly came to dominate the regions. Much the same dynamics were operative at the national level, wherein the major communal groups competed for marginal advantages and sought to form opportune communal coalitions. It became evident, however, that no coalition was stable and that no group could feel secure in the Federal system.

In the course of this communal competition for control of regional and national governmental institutions, communal minorities rapidly became disenchanted with the prevailing institutional structures. Significantly, in the light of the Biafran secession, even communal groups which were dominant at the regional level (i.e., the Ibos, Yorubas and Hausas) were in a minority position with respect to the national institutions and hence shared the same sense of communal insecurity as was characteristic of the regional minority groups. The important point is that at both the federal and the regional levels, governmental institutions became identified with the interests of particular communal groups, thereby losing their legitimacy and institutional effectiveness for crucial elements of the wider society.

Proposition 7. The greater the number of equally powerful communal groups, the greater the likelihood that political institutions will be able to retain their coherence and impartiality vis-a-vis communal competitors.

As in the case of non-political institutions, so in the political realm there is a significant reciprocity between institutions and conflict. While the political institutions of any society directly affect the nature and intensity of communal conflict, so will institutional development itself be influenced by the pattern of communal formation and conflict characteristic of that society. In particular, it would appear that the *greater the number of equally powerful communal groups*, the greater the likelihood that institutional coherence and impartiality will be retained.

On the one hand, the dispersion of power among a number of communal competitors makes less likely the identification of political institutions with a particular communal group or a coalition of such groups. On the other hand, the dispersion of power among a number of communal competitors helps not only to maintain a stable communal balance, but also to encourage the development of a system of shifting communal coalitions which, in turn, lessens the intensity of communal conflict. In a society in which no communal group is sufficiently large or powerful to stand alone, it becomes necessary to form communal alliances. The greater the number of the communal competitors, the less the likelihood that the same communal groups will join together on all issues. As issues change, so do communal alliances. The resulting pattern of shifting and intersecting communal alliances will tend to mitigate the intensity of communal conflict on any particular issue.

Thus, in the Nigerian case, though there have always been enough communal groups potentially to create an ongoing pluralistic political system, the constitutional compromises of the early 1950's had the effect of excluding communal minorities from the national political system and of creating a three-person zero-sum game among the three major nationalities. As a result, there were enough states to ensure conflict along communal dimensions, and too few states to ensure competitive stability. This was the tragic structural flaw of the Nigerian federal system. Although leaders such as Dr. Nnamdi Azikiwe and Chief Obafemi Awolowo well understood this point, their advice to create a greater number of states went unheeded. By the time enough states were created to ensure the minorities a modicum of self-determination and to prevent any single communal group from wielding inordinate power, civil war had engulfed Nigeria.

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[25] See Stokely Carmichael and Charles V. Hamilton, *Black Power: The Politics of Liberation in America* (New York: Vintage, 1967).

[26] This discussion of the Nigerian experience draws heavily upon the following studies: James S. Coleman, *op. cit.;* Richard L. Sklar, *Nigerian Political Parties, op. cit.,* K. W. J. Post, *The Nigerian Federal Election of 1959* (London: Oxford University Press, 1963), and John P. Mackintosh (ed.), *Nigerian Government and Politics* (Evanston: Northwestern University Press, 1966).

[27] See Richard L. Sklar, "The Ordeal of Chief Awolowo," in Gwendolen M. Carter, (ed.), *Politics in Africa: 7 Cases* (New York: Harcourt, Brace and World, Inc.), pp. 119–166.

Proposition 8. Political institutions which encourage the participation of the masses in the recruitment of leaders tend to further politicize and intensify communal conflict.

The greater the dependence of political leaders upon the support of their communities of origin, the greater the likelihood that the political process will itself become "communalized." In a culturally plural society, the participation of the masses in the political process encourages aspirant politicians to make appeals to the most easily mobilized communal loyalties, and to define themselves primarily as the representatives of communal interests. In the process, the political order comes to be organized along communal lines.

The stimulating effect of mass participation on communal conflict can readily be seen in the Nigerian case. With the introduction of the franchise at the federal and regional levels of the governmental system, there emerged political parties such as the Yoruba-based Action Group and the Hausa-centered Northern Peoples' Congress which made very explicit communal appeals to the pride and self-interest of the Yoruba and Hausa peoples, respectively. This appeal found an immediate response and led to the rapid rise of communally based regional parties. In turn, the development of communal parties among the majority-nationality groups stimulated the emergence of communal parties among the minority groups in each region. Parties such as the Northern Elements Progressive Union, the Bornu Youth Movement, the United Middle Belt Congress, and the Nigeria Delta Congress translated communal minority dissidence within each region into political terms. Still a further illustration of this politicization of communal conflict was the attempt made by the major communal parties to increase their strength at the federal level by appealing to the dissident minorities in each of the other regions. Thus, the Yoruba-dominated Action Group, based in Western Nigeria, became in time an important minority party in the Eastern Region and in the Middle-Belt of the Northern Region.

It should be noted that at the same time that mass participation in the recruitment of political elites acts to stimulate communal appeals and hence communal conflict, so does the resulting conflict itself further the communalization of the elites. In effect, a vicious circle ensures the perpetuation of communal conflict in a participant political system: aspirant politicians make communal appeals and communal demands which exacerbate communal tensions; these tensions, in turn, encourage the recruitment of leaders who will make communal appeals and demands. Examples of this cyclical process are plentiful in the political experience of nationalist movements. Thus, the B. R. Ambedkars and Rajagopalacharis challenge the leadership of the Nehrus, the Suhartos that of the Sukarnos, and the Carmichaels and Browns that of the Rustins and Wilkins.

Such a challenge was notably successful in Nigeria, wherein democratization and mass involvement in the political process led to the gradual displacement of moderate and universalistic political elites by more militant and more communally parochial leaders. At its inception, the leadership of Nigeria's first nationalist party, the National Council of Nigeria and the Cameroons (NCNC), though drawing primarily on Ibo and Yoruba sources, encompassed all of the country's major communal groupings and espoused a pan-Nigerian nationalist ideology. But this "nationalist" period was short-lived. With the introduction of the franchise and the gradual transfer of power to Nigerian hands, communal slogans and communally-oriented political elites acquired increasing appeal. With Regionalization, in the early 1950's, the communalization of the political system was complete. The original nationalist leadership began to sound a more parochial line, and the radical pan-Nigerian nationalists were displaced by champions of communal groups. Thus, by 1963 most of the NCNC's Yoruba contingent had broken away to found a new Yoruba-based political party and, by 1964, the NCNC had to all intents and purposes become the party of the Ibos.

In sum, then, democratic regimes which encourage mass participation and competitive political parties appear to be especially subject to the communalization of political competition. Indeed, judging from the experience of post-colonial regimes, it would seem that it is communal conflict and not class conflict which is most likely to undermine fledgling democracies.

III. COMMUNAL TRANSFORMATION

Frequently it is said that communal categories and identities are the givens of social life. One can change one's occupation, but one cannot change one's communal group as readily. Recently, however, scholars have pointed out that under conditions of social mobilization and inter-group conflict, communal boundaries are indeed transformable. In point of fact, much contemporary communal conflict—whether in Nigeria, India, or the United States—is being waged not by traditional entities, but by communities which were formed in the crucible of mobilization and competition.[28] Thus, it is probably more accurate to suggest that conflict produces "tribalism" than to argue, as the conventional wisdom would have it, that "tribalism" is the cause of conflict.

In the discussion of competitive and institutional communalism we made the tacit assumption that the culture, identity, and boundaries of communal groups are formed independently of environmental forces. This clearly is an oversimplification which we now wish to drop. This section considers the various ways in which communal identities and boundaries are transformed in response to changing social and political exigencies.

Proposition 9. In an environment of social mobilization and communal competition, communal groups will tend to fuse or to expand their traditional boundaries to include groups and individuals with whom they can identify and who might prove useful allies during inter-group conflict.[29]

A number of observers have noted the emergence within urban areas of communal identities and groupings which are considerably wider in their embrace than the communities of the traditional order. This comment on the case of African urbanization is illustrative:

> A reductive process takes place in the town: the intricate mosaic of the countryside becomes simplified to a manageable number of ethnic categories. A sense of membership in a group significantly expanded in scale from the clan and lineage system of the very localized rural 'tribal' community develops.[30]

Similarly, addressing themselves to the Indian case, the Rudolphs use the term, "fusion," to describe the process whereby Indian *Jatis* (popularly known as "castes") of unequal status join to form political and economic alliances against their competitors. It is the exigencies of modern competition which provide the impetus to alliance formation: "The higher castes need numerical strength to sustain their power and status; the lower ones need access to resources and opportunities that support of the higher ones can yield."[31] As the Rudolphs note, such alliances raise the ritual status of the lower castes and thus transform their traditional identities.

Exemplifying the process of communal expansion in Nigeria is the emergence in this century of new, affectually laden, identities of "nationality," identities which reflect a modern consciousness of cultural and linguisitic unities of which traditional man was largely unaware. The evolution of "Ibo" national identity illustrates the newness of contemporary identities of nationality especially well, for in the predominantly Ibo-speaking East pre-colonial societal fragmentation was particularly pronounced. As Simon Ottenberg has observed,

> No political superstructure, such as a federation, a confederacy, or a state existed. The Ibo units remained a relatively balanced grouping of independent political structures which never developed into a large formal organization, though some units absorbed or conquered others, some died out, some fragmented, and some changed their characteristics through immigration and emigration.[32]

But the general pattern of pre-colonial fragmentation and post-colonial expansion or fusion was characteristic not only of the Ibo but also of many other loosely-knit linguistic groups which today possess a new social and political cohesiveness and self-consciousness. It was only when these groups were brought together within a new administrative framework and forced to interact —and to compete—that nationality-wide organizations and ideologies emerged. Of course, the recency of these developments notwithstanding, few will be surprised if "revisionist" Ibo (or, for that matter, Ijaw or Yoruba or Hausa) historians of the future find many more "historical" ties that explain contemporary communal cohesiveness.

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[28] See Crawford Young's discussion of the Congolese "Bangala," an example of "artificial ethnicity" produced by inter-group competition, in his *Politics in the Congo, op. cit.*, pp. 242–246. It should be noted that contemporary "Ibo-ness" and "Yorubaness," like contemporary American "Black-ness," are also new forms of communalism engendered by mobilization and competition.

[29] For a parallel analysis of the relationship between conflict and group formation, see Lewis A. Coser, *The Functions of Social Conflict* (New York: The Free Press, 1956).

[30] Anderson, von der Mehden, and Young, *op. cit.*, p. 30.

[31] Lloyd and Susanne Hoeber Rudolph, *op. cit.*, p. 100.

[32] Simon Ottenberg, "Ibo Oracles and Intergroup Relations," *Southwestern Journal of Anthropology*, 14 (Autumn 1958), 296. For an incisive discussion of the political implications of communal transformation among the Ibo, see Paul Anber, "Modernization and Political Disintegration: Nigeria and the Ibos," *The Journal of Modern African Studies*, 5 (1967), 163–179.

The process of communal expansion in Nigeria is manifest not only in the emergence of identities of nationality but also in the emergence of new, politically salient administrative identities. Again, this process is most clearly seen in the traditionally most fragmented area, Eastern Nigeria. There, the new administrative units of Native Court Area, Division, and Province incorporated traditionally autonomous communities within a common structural framework and, in the process, generated new political interests and identities which were related to, but transcended, the village and village-group boundaries of the traditional system. Thus, in the 1960's, Ibo-speaking urban dwellers typically identified themselves to new acquaintances by reference not to their traditional community of origin, but to the administrative division and province in which that community was located. It was these "geo-ethnic" identities around which intra-Ibo political conflict normally revolved, for it was only at these wide, non-traditional levels of social grouping that Ibos were sufficiently numerous to make a political impact upon the regional political system.[33]

While this pattern of administrative fusion was especially pronounced in Eastern Nigeria, it was also evident in Western and Northern Nigeria. In the latter cases, the traditional units were considerably larger and more populous, so that the new administrative boundaries tended to correspond more closely to the existing communal boundaries. Even here, however, the creation of the new administrative structures had an effect analagous to "fusion," in that competition for governmental patronage—both in colonial and in independent Nigeria—placed a premium upon communal cohesiveness. In Western Nigeria, for example, Oyos were in competition with Ekitis and Ijebus for the scarce rewards of the administrative system, and the competitive process furthered the development of Oyo unity. Inasmuch as Oyo, Ekiti and Ijebu constituted traditional entities as well as modern administrative units, intra-Yoruba competition had an especially volatile character. By contrast, the politically relevant administrative units of Iboland were essentially artificial creations commanding relatively little affect on the part of their members. Consequently, intra-Ibo political competition acquired a distinctively pragmatic and pacific character, political success being a function of coalition development among traditionally autonomous communities.

The process of fusion also operated at the level of the *regional* governments. As will be detailed below, the tri-partite administrative system developed by the British and by Nigerian leaders led to a considerable concentration of power in regional institutions. Therefore, those who lived within a given region had a direct stake in identifying themselves with those who controlled the regional apparatus of government. In particular, minority ethnic groups stood to be penalized—for example, through the denial of government patronage to their members or the non-allocation of government amenities—if they opposed the government of the day. Thus it was that some influential members of the Tiv community sought to ally themselves with the Northern Peoples' Congress, despite Tiv antipathies toward the Muslim Hausa.[34] Moreover, this tendency toward intra-regional fusion was furthered by the regional competition for control of the federal government and for federal revenues which was built into the national political system.

Proposition 10. Mobilization and competition lead also to the increasing internal differentiation of communal categories along socio-economic and ideological lines.

Under social mobilization, individuals move into new kinds of occupational and other social roles, and become differentiated by socio-economic and ideological criteria. New kinds of social identities and conflicts emerge—based upon such reference points as occupation, class, and political party. These new identities may or may not cross-cut the lines of communal cleavage. When they do, they may produce political divisions which—in given situations—override communal loyalties.

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[33] The concept of "geo-ethnicity" has much the same meaning as Immanuel Wallerstein's concept of "ethnicity," in Wallerstein, *op. cit.* The former term is preferred here for two reasons. First, it offers a sharper conceptual tool than that of "ethnicity" to describe the new kinds of urban groupings which are based upon non-traditional or neo-traditional communities of origin. Second, the use of the "geo-ethnicity" concept to describe urban groupings based upon new, artificial entities enables us to reserve the concept of "ethnicity" for urban groupings based more strictly on kinship, cultural and linguistic ties. See Wolpe, *op. cit.*, and Wolpe, "Port Harcourt: A Community of Strangers—The Politics of Urban Development in Eastern Nigeria" (unpublished Ph.D. dissertation, Massachusetts Institute of Technology, 1965).

[34] See Martin J. Dent, "A Minority Party—the UMBC," in John F. Mackintosh (ed.), *Nigerian Government and Politics* (Evanston: Northwestern University Press, 1966), pp. 461–507.

The Nigerian experience offers two major illustration of politically significant intra-communal differentiation based on social class and ideology. The first illustration refers back to the immediate aftermath of World War II and the emergence, on the one hand, of a national trade union movement and, on the other, of the radical Zikist Movement. The second refers to the emergence of what Ricard L. Sklar has called, the "political class."[35]

In 1945 and again in 1964, successful general strikes demonstrated conclusively that the Nigerian labor movement had come of age and was capable of united and effective action. In both instances, communal divisions were cast aside and a united labor front was able to win significant concessions from both governmental and private employers. Somewhat less successful was the short-lived radical Zikist Movement, organized in the late 1940's by militant trade unionists and intellectuals, most of whom were Ibo, to rejuvenate the NCNC and to force the pace of the independence movement.

With the administrative regionalization of the country in the 1950's, a nationalist party like the NCNC, which until then had been unitarist in its constitutional outlook, had to take part in elections on the regional level. This meant that within the NCNC the fortunes of politicians and groups which were communally well connected via the tribal unions stood to rise, while the fortunes of labor activists and radical intellectuals were destined to fall. Believing constitutional advances to be a subterfuge for the protection of communal interests, and convinced that independence would come only by way of revolution, the more radical wing of the unitarists responded to the dilemmas of regionalism with a call to direct action. Revolution would put an end to constitutional subterfuge, weaken the hand of the communal faction in the NCNC, and accelerate the pace of independence.

However, the revolutionary aspirations of the labor activists and Zikists were thwarted. Moreover, unable to rely upon the personal support of Azikiwe, the Zikists were cut off from the main body of the NCNC. Many subsequently turned away from the party to Marxism, some out of conviction and some out of friendship for those who were convinced. Not until the strike of 1964 did the Zikists' isolation from Nigerian politics come to an end. From what they considered to be the NCNC's earlier betrayal and from their own obvious decline in power during the regionalist phase, the activists had learned to distrust the communal parties and their leaders and to be unalterably opposed to regionalism. Regionalism, in their view, was the structural cause underlying their political demise of the 1950's and early '60's.

The second manifestation of intra-communal diversification was the emergence of a privileged Nigerian political class whose security was tied to the maintenance of regional bases of power. As Sklar has noted, the accelerating political crises which led up to the military coup of January 1966—indeed, the coup itself—can all be viewed as phases of a continuing confrontation between this political establishment and a collection of anti-regionalist dissident groups, the latter including increasingly disaffected communal minorities and the long-alienated radical trade unionists and intellectuals.[36]

Developments such as these—the appearance of ideologically toned political manifestos and organizations, and the formation of a privileged political class—have been cited by some students of non-Western societies as evidence of the deterioration of communal identities and institutions. Indeed, implicit in many of the dichotomous models which have been employed in studies of non-Western development is the notion that communally "particularistic" and socio-economically "universalistic" roles and identities lie at the opposite end of a single social continuum, the emergence of the latter heralding the disappearance of the former. As Propositions 11–14 below suggest, this view of social development possesses neither theoretical nor empirical foundation.

Proposition 11. Accompanying communal fusion and expansion is the deparochialization of traditional group ties and perspectives.

As traditional communal entities are fused to form new and more organizationally effective groupings, the content and significance of traditional identities are substantially changed. Changes of particular importance are the generally reduced affect associated with the traditional order and the widening of social and political horizons. The Rudolphs' description of these developments in India is applicable also to Nigeria:

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[35] On the Zikist Movement, see Sklar, *Nigerian Political Parties, op. cit.*, pp. 72–82. For a discussion of the emergence of Nigeria's political class, see Sklar, "Contradictions in the Nigerian Political System," *The Journal of Modern African Studies*, 3 (1965), 201–213.

[36] See Sklar, "Nigerian Politics in Perspective," *Government and Opposition*, 2 (July-October 1967), 524–539.

> . . . As the block and district headquarters, the extravillage enterprise and the market town, the local school, cinema, and radio set, have become increasingly relevant for village lives, they have created alternative environments for profit, prestige, and self-esteem. By enlarging the reach of empathy, broadening horizons, and multiplying reference groups, they have helped to deparochialize the intimate and closed world of the village. New opportunities, sentiments, and ideas have reduced and dispersed the concentration of affect, power, and economic dependence at the local level.[37]

In short, social mobilization leads to the widening of social horizons and the loosening of traditional bonds. Urbanization, for example, removes individuals from the constraints of traditional authority and leads them to participate in numerous social rôles which not only have no traditional sanction but also which often involve interaction across the communal boundaries of the traditional order. As a result, new interests and loyalties and authorities come to have a claim upon the individual's time and attention. This does not mean that traditional identities and institutions disintegrate; to the contrary, they may well persist and retain both social and political significance. It does mean, however, that such identities and institutions, if they persist, will acquire new functions and new meaning.

From the standpoint of conventional theory, it would seem paradoxical that deparochialization has been associated with intensified rather than diminished communal conflict. Too frequently, political development has been seen in terms of a struggle between "nationalism" and "traditionalism," the fall of the latter thereby signalling the rise of the former. In point of fact, the developmental process yields many new and competitive communal groupings, of which the nation-state is but one variant, and one, moreover, which is relatively remote. Yet, to note the tenuousness of nation-state loyalties is not to denigrate the significant integrative processes at work at the regional and sub-regional levels of many polities. Indeed, it might well be argued in the Nigerian case that the very success of lower-level communal integration has made more difficult the task of national integration.

Proposition 12. Communal transformation entails the multiplication rather than the substitution of social identities.

The emergence of new patterns of communal identity through expansion, fusion and deparochialization does not necessarily mean the destruction of old communal loyalties. It means, more commonly, the superimposition upon the traditional reference points of wider concentric circles of communalism. The old identities, in effect, coexist with the new, with the consequence that citizens come to hold multiple communal identities (for example, to their family, village, region, nationality, church, nation-state). By the same token, the internal differentiation of communal categories by socio-economic and ideological criteria signals the proliferation of additional social reference points rather than the substitution of the new for the old. An individual may be an Ibo and also a factory worker. His acquisition of a new socio-economic identity need not mean the elimination of his prior communal point of reference. This point is crucial, for it explains, in part, the paradoxical persistence of communal sentiments and communal conflict within modernizing and modernized societies. The precise relationship between communal and non-communal identities is the subject of the next set of propositions.

IV. COMMUNAL COMPARTMENTALIZATION

Proposition 13. Since politically salient identities are situationally specific, communal and non-communal political orientations may well coexist within the same person.

The individual's perception of the social situation determines which of his multiple identities will be most politically salient at any given moment. The relative political salience of an individual's multiple social identities is not fixed. It is, rather, in a constant state of flux. As Anderson, von der Mehden and Young state, "Every actor in the political community, no matter how unimportant, has a multiplicity of potential foci of social solidarity. The appropriate role is prescribed by the nature of the situation—or more precisely, by the actor's perception of the situation."[38] This formulation highlights the element of contingency which underpins the always-shifting lines of political cleavage. In some situations, men join together in defense of their common religious commitment. In others, they organize to protect their economic interests or, at election time, to promote the interests and prestige of their communities of origin. The crucial point is that political response is inexplicable without reference to the actor's social context and to his perceptions of that context.

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[37] Lloyd and Susanne Hoeber Rudolph, *op. cit.*, p. 101.

[38] Anderson, von der Mehden and Young, *op. cit.*, p. 60.

This theoretical position relies heavily on the work of scholars associated with the Rhodes-Livingston Institute, in particular, Max Gluckman, A. L. Epstein and J. Clyde Mitchell.[39] Thus Gluckman has pointed out that "though developments in urban and in rural areas affect one another . . . the specific associations of each may exist independently . . . the Africans' lives are partly dichotomized, and they live in separate compartments like other men."[40] Though in this context Gluckman emphasizes the urban-rural dichotomy, in principle it can be argued without doing violence to his major point that men "compartmentalize" their identities *within* urban and rural areas as well as between them.

Since the various identities of any given individual are each "triggered" by different social situations, seemingly incompatible or conflicting identities may well co-exist within the same person. Social roles may, in effect, be "compartmentalized," thereby permitting the individual to respond flexibly to changing social and political circumstances. Thus, it is possible for the same person to join a trade union to advance his occupational interests, a communal association to promote his social or electoral objectives, and a religious interest group to lobby for educational reforms. In short, it is possible for an individual to be both a communal and a non-communal political actor.

Returning to the Nigerian case, we are now in a position to explain both the success of the 1945 and 1964 general strikes and the failure of the trade union leaders, in 1964, to convert their labor following into votes for labor candidates at election time.[41] When it came to questions of the pocketbook—to questions of salary and conditions of service—Nigerian workers held much in common irrespective of their diverse communal attachments. During the strike actions in both 1945 and 1964 it was the socio-economic identities of these men which were in the foreground; in the confrontation with employers, their communal points of reference held little salience and labor unity was possible. But when it came to the election of parliamentary candidates in 1964, their communal identities were "triggered" once again and the trade unionists voted their communal loyalties. As a result, the same trade union leaders who had enjoyed the support of a communally heterogeneous rank-and-file during the 1964 general strike were deserted when they subsequently transformed themselves from leaders of a socio-economic protest into parliamentary candidates in opposition to the communally-based regional parties. The moment the strike was concluded, the lines of political cleavage within the nation were redrawn, socio-economic identities once again being subordinated to the communal identities of region and nationality.

Proposition 14. Since communal and non-communal political orientations may co-exist within individuals, each activated in different social contexts, modernization need not eliminate communal differentiation and conflict.

A clear implication of the argument advanced above is that there is no necessary incompatibility between functioning as a modern economic man, on the one hand, and performing as a communal political actor, on the other. We have suggested, in brief, that one can participate effectively in a communally heterogeneous industrial occupation and trade union five days a week, and still vote his communal loyalties at election time. If this is so, it would appear to follow that communal differentiation and conflict may well persist in the midst of economic modernization. Not only may communal particularism be compatible with a modern industrial order, the foregoing suggests that it may well be an essential component of that order. For, by the multiplication and compartmentalization of both communal and non-communal identities, individuals are able to respond more flexibly to changing social and political circumstances. In effect, the retention of communal points of reference enables industrial man to receive the best of two worlds—the collectivism inherent in the functionally diffuse ties of language and culture, and the individuation requisite to effective performance in the modern commercial and industrial milieu. We are suggesting, in short, that the essence of modernity may lie not in the transition from particularistic to universalistic forms, but rather in their compartmentalization.[42]

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[39] See Max Gluckman, "Tribalism in Modern British Central Africa," in Van Den Berghe, *op. cit.*, pp. 346–360. This article first appeared in *Cahiers D'Etudes Africaines*, 1 (January 1960), 55–70. Also, A. L. Epstein, *Politics in an Urban African Community* (Manchester: Manchester University Press, 1958); and Clyde Mitchell, *The Kalela Dance* (Manchester: Manchester University Press, 1956). For a recent application of the situational mode of analysis developed by the Rhodes-Livingstone group to the Nigerian scene, see Leonard Plotnicov, *Strangers to the City: Urban Man in Jos, Nigeria* (Pittsburgh: University of Pittsburgh Press, 1967).

[40] See Gluckman, *op. cit.*, p. 359. For a critical discussion of Gluckman's views, see Michael Banton, *Race Relations* (New York: Basic Books, 1967), pp. 239–240.

[41] See Robert Melson, "Politics and the Nigerian General Strike of 1964," in Robert Rotberg and Ali Mazrui (eds.), *Protest and Power in Black Africa* (New York and London: Oxford University Press, 1970); and Wolpe, "Port Harcourt: A Community of Strangers," *op. cit.*, pp. 423–454.

How well individuals and groups do in fact compartmentalize their many identities is of course an empirical question. It can be expected that the extent to which co-existing social identities will be compartmentalized will vary with how individuals and groups perceive and interpret particular situations. There arise situations, for example, in which individuals may experience "role conflict" or "cross-pressures" which reflect a difficulty in choosing among conflicting loyalties or in determining which set of loyalties is most appropriate to a particular situation.[43]

An illustration of this problem can be gleaned from the experience of Nigerian workers after the June General Strike of 1964 and before the December Federal elections. Two kinds of politicians began to compete for the labor vote. On the one hand, there were politically-oriented labor leaders who wanted to take the opportunity of the strike and of the elections to form a labor party. They made their appeals to the class interests of Nigerian workers. On the other hand, there were the politicians of the major political parties who made their appeals to their communal loyalties. Thus between June and December 1964, workers were cross-pressured between their class and their ethnic loyalties. As it turned out the cross-pressure was weak and a majority of workers supported their communal parties.

How the workers responded to the cross-pressure was clearly related both to the objective situation of growing communal conflict during the election and to their perception of the incompatibility between choosing a labor party and choosing a communal party. With regard to the first point it should be noted that the Federal election of 1964, coming as it did on the heels of the many political crises of Federation, raised the salience of communal security and survival. We should not be surprised therefore that at first, in June, almost nine out of ten workers responding to a survey indicated support for a labor party, but that as the election progressed a growing number of workers indicated support for regional parties and diminishing support for labor parties. With regard to the second point we should not be surprised that almost a fifth indicated their allegiance to *both* parties throughout the election period. Such inconsistency need not have been based on flaws in logical reasoning, but on the confusing situation which existed during the election. It is significant, for example, that the more educated the trade unionist and the longer his involvement in the trade union—that is, the more information the worker had at his disposal—the less likely was he to be inconsistent and to support both parties.[44]

In order to understand the political behavior of actors who are cross-pressured between their communal and non-communal identities it becomes necessary to specify to what extent roles are compartmentalized and to what extent cross-pressures are actually felt. This empirical variance among individuals and between situations does not detract, however, from the basic point that there need not be an inherent incompatibility among co-existing communal and non-communal identities.

V. CONCLUSIONS

In concluding this analysis of the relationship between modernization and communal conflict, three themes require emphasis. The first concerns the political distinctiveness and significance of communal conflict, as contrasted with other types of inter-group conflict. Clifford Geertz has noted that communal groups differ from groups formed on the basis of economic interest or "class-consciousness" in that the latter are seldom ". . . considered as possible self-standing, maximal social units, as candidates for nationhood."[45] Since communal groups may in

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[42] As the Rudolphs note, "Compartmentalization not only physically separates . . . home and family from place and colleagues, but also prevents the different norms of behavior and belief appropriate to modernity and tradition from colliding and causing conflict in the lives of those who live by both." *Op. cit.*, pp. 121–122.

[43] "Cross-pressures are combinations of characteristics which in a given context would tend to lead the individual to vote on both sides of a contest." See Bernard Berelson *et al.*, *Voting* (Chicago: University of Chicago Press, 1954), p. 283.

[44] See Robert Melson, "Ideology and Inconsistency: The Politics of the 'Cross-pressured' Nigerian Worker," *American Political Science Review* (March 1971), reprinted in Melson and Wolpe, *op. cit.*

[45] Geertz, *op. cit.*, p. 111. See also Deutsch, *Nationalism and Social Communalization*, *op. cit.*, p. 78: "In an age of nationalism, a nationality is a people pressing to acquire a measure of effective control over the behavior of its members. It is a people striving to equip itself with power, with some machinery of compulsion strong enough to make the enforcement of its commands sufficiently

fact be considered as "candidates for nationhood," communal conflict—unlike, for example, class conflict—may bring into question the very boundaries of the nation-state.[46] This is not to suggest that class conflict may not possess the same intensity as communal conflict: the Russian and French revolutions testify to the potential intensity of class conflict. Nor is it to suggest that communal conflict may not be significantly motivated by socio-economic issues; on the contrary, to the extent that communal conflict touches on problems of redistribution of wealth, status, and power it tends to resemble class conflict. The important point, however, is that conflict between social classes which are not also communal groups does not threaten the integrity of national political boundaries, whereas the very nature of communal conflict involves the potentiality of secession and boundary revision. Of course, to the extent that modernization uproots communal groups from particular geographical areas and distributes their members according to the exigencies of the market place, the probability of actual secession is greatly reduced. Thus, secession did not prove a viable strategy for pre-war European Jews, despite the fact that intense communal conflict produced minority demands for geographical and political separation. Indeed, it was from such roots that the Zionist movement sprang.[47] For similar reasons, the chances for secession by Black Americans in the contemporary United States are slim indeed, though the Black Muslim and Republic of New Africa movements are eloquent testimony to the intensity of separatist sentiment within some segments of the Black American community.[48] It should be noted that the foreclosure of the secessionist option is one of the factors underlying the sense of powerlessness of communal minorities in modern societies.

The second critical theme which bears elaboration is the persistence of communalism in transitional and modern as well as traditional societies. This is so because communal formation and conflict are not merely the reflection of cultural "givens" and "primordial sentiments."[49] Social change leads to the formation of entirely new communal groupings which crystallize around new foci of culture and identity. Communalism, in short, is an inherent aspect of social change in all culturally heterogeneous societies. This suggests that the disappearance of those groups which comprise a culturally heterogeneous society at any particular point in time herald neither the disappearance of all communal formations nor the amelioration of communal conflict. Thus, in the United States, the transformation of first-generation Italian, Irish, and Polish immigrant identities and institutions has signalled not the disappearance of communal politics, but the emergence of a more inclusive communalism based upon ethnic solidarity on the one hand, and religious denominationalism on the

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probable. . . ." For Deutsch, what differentiates a people from a nationality is precisely the attempt by a communal group to gain power in order to discipline itself and to have its demands met by others. In that sense, Nigerian political parties, to the extent that they became representatives of a particular communal group served the function of transforming a people into a nationality. Thus, by 1965, Nigerian politics had come to resemble the politics of the multi-national nation-states of Europe before the First World War. See also Anderson, von der Mehden and Young, *op. cit.*, p. 17: "By 'nationalism' we mean the assertion of the will to constitute an autonomous political community by a self-conscious group whether or not the group coincides with a recognized state."

[46] It is interesting that many scholars have tended to discount the significance of communal conflict. Ralf Dahrendorf, for example, while distinguishing between communal and class conflict, dismisses consideration of the former in favor of an analysis of the latter. In formulating a theory of class conflict, he asserts, ". . . we are by no means considering a general theory of social conflict, although I would undertake to defend the assertion that we are dealing here with one of the most important, if not the most important type of social conflict. However important as problems of social conflict St. Bartholomew's Night, Crystal Night, and Little Rock may be [these are all examples of communal conflict], the French Revolution, the British General Strike of 1926, and the events in East Berlin on June 17, 1953, seem to me more germane for structural analysis . . . the sociological theory of conflict would do well to confine itself for the time being to an explanation of the frictions between the rulers and the ruled in given organizations." See Dahrendorf, "Toward a Theory of Social Conflict," in Amitai Etzioni (ed.), *Social Change* (New York: Basic Books, 1964), p. 101.

[47] On the Zionist movement, see Ben Halpern, *The Idea of the Jewish State* (Cambridge: Harvard University Press, 1961).

[48] On the Black Muslim movement, see E. U. Essien-Udom, *Black Nationalism: A Search for Identity in America* (Chicago: University of Chicago Press, 1962).

[49] Geertz' term, "primordial sentiments," may be an unfortunate label, if it suggests that the bonds which define the cohesiveness of a communal group are in some sense pre-historical, given, or unchangeable. *Op. cit.*, p. 109.

other hand.[50] Similarly, in Nigeria, the weakening of traditional authority and cohesiveness has been accompanied not by the disappearance of communal identities but by their transformation and expansion.

Finally, the potentially disintegrative implications of intense communal conflict, together with the likelihood of its persistence, suggest that policy makers must begin to come to terms with communalism. If we are correct in our analysis of the relationship between modernization and communalism, communal categories might well become more rather than less politically salient. Intense communal conflict poses an especially trying dilemma for fragile democratic regimes. To the extent that such regimes encourage social mobilization and participation, they may be undermining the very basis of their legitimacy and stability. Political arrangements must be found which accord to all communal groups a meaningful role in national life and which are able to keep communal conflict within manageable bounds. The stability of culturally plural societies is threatened not by communalism, per se, but by the failure of national institutions explicitly to recognize and accommodate existing communal divisions and interests.

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[50] Michael Parenti, "Ethnic Politics and the Persistence of Ethnic Identification," this REVIEW, 61 (September 1967), 717–726.

SOLDIERS IN MUFTI: THE IMPACT OF MILITARY RULE UPON ECONOMIC AND SOCIAL CHANGE IN THE NON-WESTERN STATES[1]

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When military officers are either sitting in the governmental saddle or have one foot securely in the stirrup, is it likely that such military controlled governments will pursue policies of socioeconomic change and reform? What are the officer-politicians' motivations in reacting to the possibilities of such modernizing changes? Under what conditions are their motivations likely to vary? This essay attempts to answer these questions with regard to the contemporary non-western states. And in making the attempt, I believe that the analysis falls squarely within the purview of certain recent changes that are taking place in the study of comparative politics. These changes may be most broadly depicted as a movement away from that aspect of behavioralism that has focused exclusively upon "inputs," and away from that dimension of "scientism" that has focused upon abstract concepts at the expense of empirical analysis. The change can also be described (in an overly facile manner) as a movement toward the politics in political science and the government in comparative politics.

As is evidenced in LaPalombara's call for "parsimony" in the selection of problems, we should choose problems for analysis that are blatantly political and of obvious contemporary relevance. In approximately half of the contemporary non-western states military officers either occupy the topmost seats of government themselves or they have a marked influence upon the civilian incumbents. And when this fact is placed alongside the potential of most contemporary governments to influence the pace and direction of social and economic change, this essay's central concern fulfills LaPalombara's criterion. The shift in emphasis is also found in Pennock's concern with those normatively defined outcomes which he calls "political goods," and Almond's recent emphasis upon governmental capabilities. Insofar as governments act or fail to act in satisfying popular aspirations for greater economic opportunities, security and well-being, we are in the realm of Pennock's "political goods" of individual security and dignity, and what Almond refers to as the government's "responsive" and "distributive" capabilities. Huntington's singular concern for political and governmental institutions, and Macridis' desire for a comparative politics that is relevant to, and focused around, the "black box" of governmental institutions, represent another aspect of the "winds of change" that are beginning to reshape the study of comparative politics. Since this essay is not directly concerned with the factors that contribute to military intervention, focusing instead upon the officers after they have translated their military power into governmental power, we are clearly well within the confines of the governmental "black box." And although I do not intend to introduce the concept of institutionalization in this essay, the appearance of soldiers in mufti is an important manifestation of Huntington's notion of low political institutionalization or "political decay."[2]

I. THE STATE OF THE LITERATURE

A number of respected political scientists have already offered a substantial answer to our central question. According to this perhaps prevailing interpretation, the likely consequences of military rule are economic growth, the modernization of economic and social structures, and a more equitable distribution of scarce economic

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[1] The research and writing of this article was generously supported by the Center for International Affairs, Harvard University, and the National Science Foundation (grant number 01-GS-2098). For their much appreciated suggestions and criticisms I am indebted to Amos Perlmutter, Gary Orran, John D. Powell, Donald Hindley, George A. Kelly, and James F. Guyot.

[2] Joseph LaPalombara, "Macrotheories and Microapplications in Comparative Politics: A Widening Chasm," *Comparative Politics,* 1 (October 1968), 52–78; J. Roland Pennock, "Political Development, Political Systems, and Political Goods," 18 *World Politics* (April 1966), 415–434; Gabriel A. Almond, "A Development Approach to Political Systems," 7 *World Politics* (January 1965), 183–214; Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, 1968); Roy C. Macridis, "Comparative Politics and the Study of Government," *Comparative Politics,* 1 (October 1968) 79–90.

values and opportunities. As sponsors of these types of change, soldiers in mufti are depicted as progressive forces, whose politicization is to be commended if not recommended, rather than being condemned as an usurpation of civilian authority. A number of factors are said to have shaped the officers' progressive motivations in carrying out the *coup* and in governing the country in the post-*coup* period.

Lucian Pye has suggested that the continuing modernization of the military's organization and weaponry has instilled in the officers the belief that their society ought also to be modernized. "Above all else the revolution in military technology has caused the army leaders of the newly emergent countries to be extremely sensitive to the extent to which their countries are economically and technologically underdeveloped. Called upon to perform roles basic to advanced societies, the more politically conscious officers can hardly avoid being aware of the need for substantial changes in their own societies."[3] Similarly, with respect to the Middle East and North Africa, Manfred Halpern has written: "the more the army was modernized, the more its composition, organization, spirit, capabilities, and purpose constituted a radical criticism of the existing political system."[4] With reference to Latin America, John J. Johnson has pointed to a related consequence of military modernization: the officers' technical-managerial orientation is said to have made it easier for them to accept the shift in political power from the land-holding elite to the new "urban alliances."[5]

The officers' social backgrounds are also thought to be relevant. Edward A. Shils has noted that in non-western societies officers tend to be recruited from lower middle class families of traders, craftsmen and small farmers, and that these men are painfully aware of the distance separating them from the wealthy and powerful. When these officers do achieve political influence they are not sympathetic to big businessmen and conservative politicians, and thus by implication, they are favorably disposed toward the redistribution of wealth.[6] With respect to Latin America, Johnson has claimed that the increasing recruitment of officers with lower middle class and working class backgrounds means that "the armed forces may be expected to be more inclined than formerly to gravitate toward positions identified with popular aspirations and to work with the representatives of the popular elements." Furthermore, as officer recruitment penetrates to lower social strata, "the industrial-commercial bourgeoisie in Latin America will be surrendering control of the armed forces, which are maintained by their taxes, to groups more radical than themselves."[7]

In referring to the officer corps of Southeast Asia, Guy Pauker states that they "are not the product of social classes with feudal traditions." Rather, their participation in the struggles for national independence and modernization, has produced an officer corps that is unlikely to become "the natural allies of feudal or other vested interests. Their natural propensities are progressive."[8] Then there is Halpern's contention that in the Middle East and North Africa, the officers showed "an acute awareness of the chronic ills of their countries," having joined the military in order to escape from the economic frustrations of civilian life compounded by the established elites' failure to encourage economic growth.[9]

This last comment of Halpern's is only one part of his powerfully stated argument in which officer-politicians act as effective agents of socio-economic change because of their close connection with "the new middle class." The defining feature of this new middle class is its salaried position, in contrast with the propertied and land-owning middle class. It is made up of teachers, administrators, scientists, lawyers, engineers, while collar workers and military officers. This class is said to be commited to the refashioning of society; it is only through social reforms that careers will be opened and secured for people like themselves who constitute a meritocracy rather than an established class. The new middle class is sufficiently self-conscious and powerful to "undertake the remolding of society," in large part because it has enlisted the military in this effort. "The army has become the instrument of the new middle class." And in adhering to this alliance the army has been transformed "from an instrument of repression in its own interests or that of kings into the vanguard of nationalism and social reform."[10]

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[3] Lucian W. Pye, "Armies in the Process of Political Modernization," in John J. Johnson (ed.), *The Role of the Military in Underdeveloped Countries* (Princeton, 1962), p. 78; also see pp. 80–82.

[4] Manfred Halpern, *The Politics of Social Change in the Middle East and North Africa* (Princeton, 1963), p. 258. Similarly, see Morroe Berger, *The Arab World Today* (New York, 1962), pp. 389–390, and P. J. Vatikiotis, *The Egyptian Army in Politics* (Bloomington, 1961), esp. pp. 211, 233.

[5] John J. Johnson, *The Military and Society in Latin America* (Stanford, 1964), p. 237.

[6] Edward A. Shils, "The Military in the Political Development of the New States," in Johnson (ed.), *op. cit.*, p. 17. But cf. p. 31 for the soldiers' distrust of anti-traditional modernizers.

[7] Johnson, *op. cit.*, pp. 152, 250. Also see Lyle N. McAlister, "The Military," in John J. Johnson (ed.), *Continuity and Change in Latin America* (Stanford, 1964), pp. 145–160.

[8] Guy J. Pauker, "Southeast Asia as a Problem Area in the Next Decade," *World Politics,* 11 (April 1959), pp. 339–340.

[9] Manfred Halpern, "Middle Eastern Armies and the New Middle Class," in Johnson (ed.), *The Role of the Military . . . ,* p. 295.

Since this essay is an attempt to establish the plausibility of an alternative interpretation of the consequences of military rule I should like to make two points quite clear at the outset. First, I am not going to take up the question of whether the particular reasons these writers offered for their conclusion that the military in politics are agents of economic change are valid or invalid. In passing, I would say that they have presented remarkably little evidence for their arguments, while failing to analyze—as opposed to simply stating—the supposed connections between the officers' technical orientations and social backgrounds and their hypothesized modernizing activities and motivations.[11] Even if they are correct in thinking that the officers' technical orientations and anti-conservative social backgrounds do predispose them to carry out programs of economic and social change, other factors have been ignored which have a sufficiently powerful effect upon the officers' motivations to overshadow their possible modernizing predispositions.[12] And where the military do sponsor certain types of economic change they do so for reasons which are quite different from those that have been suggested. Secondly, a number of political scientists, among them Shils, Pye, and Halpern, believe that a country's military establishment is likely to have important modernizing impacts over and above those already alluded to. Hans Daalder presents the following list of such consequences: military considerations have dictated the building of roads, communications networks and heavy industries, which have "benefited social and economic life generally;" the skills learned within the military may be applied to civilian pursuits after the officers and men leave the service; and the rational basis of military organization has a spillover effect upon organizational patterns throughout the society.[13] My reaction to this list of happy outcomes is simply this: the military may or may not be making these contributions, but whether they do or do not is almost completely unrelated to their acquisition of *political* power. The validity of Daalder's generalizations is at best marginally affected by the presence or absence of military officers in decision-making circles. Which is to say that these generalizations are beyond this essay's confines.

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[10] Manfred Halpern, *Social Change* . . ., pp. 52–54, 253, 258 and *passim*. See the criticisms levelled at Halpern's thesis in Amos Perlmutter, "Egypt and the Myth of the New Middle Class: A Comparative Analysis," 10 *Comparative Studies in Society and History* (October 1967). The ensuing debate between Perlmutter and Halpern is found in *ibid.*, 11 (January 1969) and 12 (January 1970).

[11] Although he is only referring to Latin America, Schmitter's comments are even more applicable to those continents in which military intervention is not quite the hallmark it has become in Latin America. Studies of military intervention in Latin America have "surprisingly . . . focused exclusively on (its) *causes* . . . and have neglected almost entirely (its) *consequences*. They leave us with the generals (or colonels as the case may be) battering down the gates to the presidential palace . . . and tell us very little about what these triumphant groups do with their newly acquired power." The officer-politicians become the "objects rather than the subjects of analysis." The literature abounds with speculations and scattered observations regarding the consequences of military intervention, but they "are rarely accompanied by evidence." Relying upon cross-national and longitudinal aggregate data, Schmitter systematically analyzes the impact of Latin America's officer-politicians upon governmental outputs and economic outcomes, although he does not attempt to explain the officers' political behavior. For the most part his findings dovetail with the aggregate analysis reported below. Philippe C. Schmitter, "Military Intervention, Political Competitiveness and Public Policy in Latin America: 1950–1967," mimeographed, 1970, Center for International Affairs, Harvard University, pp. 1, 7.

[12] Morris Janowitz also suggests that the officers' technical education and their opposition to religiously defined traditions *inclines* them to act as modernizers. However, he does not argue that these predispositions are sufficiently pervasive and powerful to overcome other factors, which would allow him to say that soldiers in mufti generally *act* as modernizers. See *The Military in the Political Development of New Nations* (Chicago, 1964), pp. 28, 44, 67.

[13] Hans Daalder, *The Role of the Military in the Emerging Countries* (The Hague, 1962), pp. 18–20. For similar claims, including socialization into national as opposed to parochial attitudes, see Janowitz, *op. cit.*, pp. 81–82; M. J. V. Bell, "The Military in the New States of Africa," in Jacques Van Doorn (ed.), *Armed Forces and Society* (The Hague, 1968), pp. 263–264; Lyle N. McAlister, *op. cit.*, pp. 136–144; Johnson, *op. cit.*, p. 258.

Returning to the questions with which I began this paper, my answers to them can now be set out. At the level of descriptive generalization, when military officers occupy the highest seats of government, or when they have a good measure of control over the civilian incumbents, the officer-politicians are commonly unconcerned with the realization of economic change and reform, and where there are civilian organizations and strata pressing for such changes the officers purposefully oppose them. This statement must, however, be immediately modified by adding that within a particular social and political context (when there is hardly a middle class to speak of, and when workers and peasants have not been politically mobilized), soldiers in mufti sometimes allow or even encourage economic modernization. At the level of explanation, I would point to two motivations that go a long way toward accounting for the officer-politicians' conservative tendencies in protecting or failing to alter the economic and social *status quo*. These are the officers' determined pursuance of their corporate interests in combination with their deeply inculcated military values that assign overriding importance to the preservation of a particular type of political stability, and the officers' attachments to their middle class interests and identities. This interpretation does not include the claim that regimes featuring a politicized military are necessarily less successful in bringing about modernizing changes than are regimes of a purely civilian variety. I am only suggesting that, except under certain conditions, soldiers in mufti are not agents of modernization as has been commonly asserted, concomitantly offering some explanations for this descriptive generalization. And even if it turns out that civilian politicians fare no better as modernizing agents than officer-politicians, the motivations and values underlying the activities of the latter are commonly quite different from those of the former.

II. THE OFFICERS' CORPORATE INTERESTS AND MILITARY VALUES

Dankwart Rustow offered an incontrovertible generalization when he wrote: "In the mid-twentieth century, any serious claimant to power, regardless of his antecedents, associations, or intentions, will justify his claim by professing profound concern for national independence, for popular aspirations, for social justice and for economic development."[14] In the words of another writer, "At the hour of the political triumph, all army officers in the postwar Middle East have claimed to be card-bearing reformers."[15] However, when the military are a more or less autonomous state within a state, without a constituency to which they are responsible or an executive to which they are subordinate, they are unlikely to be motivated by the goals of popular responsiveness, social and economic reforms, or economic development. The officers' corporate interests and military values go a long way in accounting for their disinclination to match their original *pronunciamientos* with governmental decisions.

In discussing the various factors which dispose the armed forces to intervene, S. E. Finer attributes a good deal of importance to the officers' corporate self-interest. "The military is jealous of its corporate status and privileges," which, in its most aggressive form, "can lead to the military demand to be the ultimate judge on all matters affecting the armed forces. . . . these certainly include foreign policy, and invariably include domestic economic policy and may well include all the factors making for morale, i.e., education and the mass media of communication . . ."[16] While I certainly agree with this point, I would like to expand upon it by suggesting how this corporate self-interest is more encompassing than implied by Finer's statement and his supporting examples, concomitantly showing how it hinders economic and social change.

When we add the enormous political power frequently enjoyed by the military to their corporate interests, it comes as no surprise to find that the armed forces have sometimes been characterized as the country's most powerful "trade union." And as is true of most unions, the military act to maintain or increase their wealth and prerogatives even when these values conflict with the aspirations and interests of larger segments within the society. In countries suffering from economic scarcity, and in the absence of internal or external threats, funds devoted to military expenditures hinder the rate of economic growth, and limit the size of progressive social service, health and welfare programs.

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[14] Dankwart A. Rustow, *A World of Nations: Problems of Political Modernization* (Washington, D.C., 1967), pp. 16–17.

[15] J. C. Hurewitz, *Middle East Politics: The Military Dimension* (New York, 1969), p. 117.

[16] S. E. Finer, *The Man on Horseback: The Role of the Military in Politics* (New York, 1962), p. 47 and *passim*.

What is more to the point here is the existence of a relationship between size of these economically and socially unproductive military expenditures and the military's political strength. In Latin America, with a sizeable proportion of governmental expenditures already being devoted to the armed forces, these expenditures were increased whenever a military government came to power.[17] In those Latin American countries with a politically non-involved military the mean level of defense expenditures as a proportion of central government expenditures is 9.3 per cent; in countries with an officer corps that has intermittently entered the political arena the percentage is 14.1; while the figure reaches 18.5 per cent in countries dominated by the military circa 1960. This last group of Latin American countries also evidenced a 14.0 percent average annual increase in defense spending circa 1960 to 1965, this figure dropping to 3.3 per cent where the military were intermittent political actors and 2.8 percent where they were politically non-involved.[18] In their exceptionally reliable compilation of aggregate data for 74 non-western, non-communist countries to which the United States has extended some foreign aid, Adelman and Morris classify these countries according to the "political strength of the military."[19] Relying upon the World Handbook of Social and Political Indicators for the proportion of these countries' GNPs spent on defense (circa 1960),[20] we find that in those countries in which the military had occupied governmental office during at least part of the 1957–62 period (N=18), the average percentage of GNP spent on defense is 3.6. This proportion is slightly lower (3.4 per cent) among those countries in which the military did not serve as governmental incumbents, but did exert a good deal of influence upon the civilians (N=20). Among those countries in which the military's activities remained circumscribed within their instrumental role (N=36), the percentage drops to 1.9. Thus the proportion of GNP devoted to defense is almost twice as large in countries overtly ruled by the military as it is in countries with a non-politicized officer corps, indicating that soldiers in mufti actively pursue their corporate interests and that they do so in a way that detracts from economic and social change.

There is good reason for the officers to view the lower classes as a real or potential threat to their corporate interests, and as such, they are not about to sanction economic programs that will benefit the disadvantaged strata. I would point to three possible factors that lead the military to perceive a politically active peasantry and urban working class as a threat to the military establishment: as an additional contender for political power within a zero-sum political arena, mass political mobilization will decrease the political strength of the military, and thus the ability to pursue its corporate interests; the lower classes are likely to attack the military establishment itself when the officers have previously opposed them in their bid for an effective political role and a larger slice of the economic pie; and the lower classes will try to reduce superfluous military expenditures in order to channel precious economic resources into the modernization process. Insofar as the officers are aware of the close connection between economic modernization and political mobilization, they have an additional reason for preserving the economic status quo. These possibilities take on some illustrative flesh by briefly noting two Latin American examples.

The Guatemalan Army leadership sponsored a strongly reform oriented *coup* in 1944, only to act against the very benefactors of that *coup* in 1954 and 1963. Lieuwen accounts for this reversal when he writes that

> the present Army leadership has turned its back on the revolution it made in 1944. It got its fingers badly burned by involvement in the upsetting process of fundamental social change and reform. The revolutionary program, the civilian leadership, the workers and peasants, not to mention the Communists, all turned out to be threats to the very existence of the armed forces. Having eliminated these threats in 1954, (the officers) took every precaution to guard against their resurrection only a decade later. For not only do they now oppose social reform in principle, but they feel they must now protect themselves against the revenge that

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[17] Edwin Lieuwen, *Arms and Politics in Latin America* (New York, 1961), pp. 147–148.

[18] Schmitter, *op. cit.*, p. 25. In terms of defense expenditures as a percentage of GNP (1960), the ordering is somewhat reversed, with the "intermittent" regimes averaging 2.7 per cent, and only 2.1 per cent of GNP being spent in military-dominated regimes. (The figure is 1.2 per cent where the military are completely out of politics.) Schmitter accounts for this pattern by noting that the civilian elites attempted to buy off the military as they moved in and out of politics. (p. 51)

[19] Irma Adelman and Cynthia Morris, *Society, Politics and Economic Development* (Baltimore, 1967), pp. 74–76. See below, p. 1138 for an explication of this three-fold division.

[20] Bruce M. Russett, *et al., World Handbook of Political Social Indicators* (New Haven, 1964), pp. 79–80.

would surely be forthcoming if Arévado and the labor-left should return to power. Thus, institutional self-preservation was a prime motive for the March, 1963, *coup*.[21]

The officers of the Dominican Republic were similarly inspired in the September, 1963 *coup* that removed Juan Bosch from office. The officers reacted against Bosch's expensive development and welfare proposals which were bound to get the lion's share of the governmental budget —an interpretation supported by the fact that it was the Air Force, with its costly equipment and whose request for $6 million of new aircraft had just been rejected by Bosch, that sparked the *coup*. In addition, the military leaders became alarmed at Bosch's encouragement of the urban and rural workers' organizational efforts, insofar as their new found power would ultimately represent a counterpoise to the military.[22] Although these two examples refer to the motivations underlying military intervention in Guatemala and the Dominican Republic, we may safely assume that if these interpretations of the *coups* are valid, the subsequent military governments would hardly be disposed to act as agents of socio-economic change.

There are, of course, instances in which a military government or a civilian government largely under the military's influence does bring about economic change and reform. But even in this minority of cases the officers' corporate interests decrease the extensiveness of such efforts. The officer-politicians may permit the government to carry out developmental and reform programs, but only if the military's corporate interests are first satisfied, the costs of the latter thereby detracting from the extensiveness of the former. For example, the Venezuelan armed forces allowed President Betancourt to serve out his entire constitutional term of office (the first time this had occurred in the country's military-dominated history) despite the implementation of agrarian reforms and a progressive tax law which redistributed both liquid and landed wealth. However, Betancourt was only able to move in these directions after he endorsed a more than liberal defense budget, even permitting the purchase of jet aircraft for the military's mythical defensive role, and a generous allotment of fringe benefits and promotions. In addition, the officers were presumably reassured that the cost of the development programs would not reduce these perquisites in the future because of the enormous oil revenues accruing to the government.[23] There is one type of economic change that officer-politicians have sometimes endorsed, *because* it enhances their corporate interests. The military often place a high value upon industrialization, both for its symbolic indication of military might and its presumed guarantee of national self-sufficiency in the event of an improbable war. Yet industrialization is not not always an especially successful strategy of economic development,[24] and given the identification of industrialization with the military's corporate interests, the officer-politicians' determined pursuit of the latter is likely to produce an over-emphasis upon industrialization. The industrializing impetus would then exacerbate the country's economic problems, as in some Latin American countries where the military rulers' "drive for economic independence often led to overhasty industrialization programs."[25] At a minimum, industrializing efforts detract from the oftentimes more desired and desirable objectives of increased agricultural productivity, land reform, educational expansion and the provision of social and medical services which are unrelated to the military's corporate interests. The military's corporate interests may positively incline them toward economic modernization, but rarely toward economic and social reform, for the latter almost never coincide with even the broadest of corporate interests.

I would underline the tenacity with which the officers pursue their corporate interests by pointing out that these are inextricably bound up with their self-image as the selfless and dedicated guardians of the nation's interests. The military's corporate interests are largely defined, legitimized and rationalized by their close identification with the nation's interests. The officers are thereby able to justify their actions to others and to themselves by identifying with the nation rather than with any of its constituent parts; opposition to their actions are viewed as expressions of partial and selfish interests. This close identification between their own and the nation's interests is so deeply inculcated that the officer-politicians readily come to believe in the necessity and legitimacy of their actions, no matter how self-interested and abusive they may appear to an outside observer. Moreover, this equation of the two interests cannot be questioned. To do so might very well entail the enfeeblement of the military establishment; for its *esprit de corps* is founded upon service, duty, and responsibility to the nation. Any challenges on this score cannot arise, or if they do, they must be rationalized away or defeated. In short, those corporate interests which block socio-economic change and popular responsiveness are strengthened and legitimized by their inextricable association with the nation's interests.

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[21] Edwin Lieuwen, *Generals vs. Presidents: Neo-Militarism in Latin America* (New York, 1964), pp. 42–43.

[22] *Ibid.*, pp. 60–61.

[23] *Ibid.*, 86–91; Philip B. Taylor, Jr., *The Venezuelan Golpe de Estado of 1958: The Fall of Marcos Perez Jimenez* (Washington, D.C., 1968), pp. 48–52, 67–71.

[24] For the persuasive argument that "The postwar dogma, so widespread among the less developed countries, that modern economic growth is dependent wholly on industrialization has done much harm," see Theodore Schultz, *Economic Growth and Agriculture* (New York, 1968), p. 21 and *passim*, where it is also suggested that the agricultural sector of most non-Western countries might contribute most to economic growth.

[25] Lieuwen, *Arms and Politics . . . , op. cit.*, pp. 146–47. Also see Jae Souk Sohn, "Political Dominance and Political Failure: The Role of the Military in the Republic of Korea," in Henry Bienen (ed.), *The Military Intervenes* (New York, 1968), pp. 114–116.

A second aspect of the military establishment that hinders economic change are those near-universal military values—the normative attachments to order, dignity and hierarchy—with which most officers are strongly imbued, out of which emerges an overwhelming concern for political stability, and thus a keen sensitivity to any divergence from the status quo that contains the potential for unwieldy change. Given the brittleness of most non-western governments, we would expect these military values to have an especially pronounced impact upon the officer-politicians' activities; where governmental institutions are weak the officers have good reason for thinking that practically any type of socio-economic change could reduce the level of stability. However, it is one thing to place an extremely high value upon stability in the context of a fissiparous politics, it is quite another when stability is merely intended to replace a cacaphonous politics. Consequently, even in those small number of instances in which a politicized military is interested in change and reform, when it becomes apparent that a small cost might have to be paid in terms of some political instability, their military values leave them little time for hesitation. Even in the estimate of John J. Johnson, who is certainly not as critical of the Latin American military as is Edwin Lieuwen, officer-politicians in Latin America might be willing to sanction land reform, but only if the cost to them is not too high and so long as it can proceed in an orderly manner.[26]

Not only does this normative and cognitive map of the political world dictate a conservative reaction to the possibilities of change, the officer-politicians are wedded to a particular form of political stability—what may be described as the creation of an apolitical calm—that further reduces any possible modernizing predispositions. The goal is political stability, "but it is the stability of a vacuum, a state undisturbed by the erratic movements of partisan bodies."[27] Politics as regulated conflict, competition and compromise is transformed into the apolitical politics of consensus, acquiescence and government by fiat. For example, the attempt to create an apolitical stability in Burma during Ne-Win's military dictatorship is manifested in the establishment of a national "political" movement with the give-away name of the National Solidarity Association. According to Zolberg, military rulers in tropical Africa

> conceive of national unity as "oneness," defined negatively by the absence of social conflict stemming from regionalism, primordial loyalties such as ethnicity or religious affiliations. In all countries, 'ethnic particularism' has been condemned and its manifestations through voluntary associations prohibited. The goal seems to be the achievement of homogeneity by political fiat, as if the rulers genuinely believed that the absence of conflict somehow *produces* national integration.[28]

The tendency of newly established military governments in tropical Africa to arrest and deport even those politicans who had opposed the previous civilian regime and favored military intervention, and their abrogation of bargaining efforts on the part of occupational groups, may also be interpreted as manifestations of their particular conception of political stability.[29] This vision of political stability hinders social and economic change insofar as such changes are a product of governmental responsiveness to articulated and forcefully promoted demands; re-pressing these demands largely rules out their fulfillment.

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[26] Johnson, *op. cit.*, p. 147.

[27] M. D. Feld, "Professionalism, Nationalism, and the Alienation of the Military," in Doorn (ed.), *op. cit.*, p. 68.

[28] Aristide R. Zolberg, "Military Intervention in the New States of Tropical Africa," in Bienen (ed.), *op. cit.*, p. 87. Zolberg goes on to say that the military rulers "think much like their predecessors in the one-party states." However, the impression that I derive from his essay, combined with some knowledge of African single-party states, is that the military do place greater emphasis upon governmental suppression of actual and potential conflict groups.

[29] *Ibid.*, pp. 89–90.

Although I am attributing a good deal of importance to these two broad aspects of the military, they do vary in their explanatory power. Soldiers in mufti do not pursue their corporate interests in as determined a fashion where the military establishment is a small one. Where the armed forces number less than 2,000 men we would not expect the "weight" of such a small establishment to have as pronounced an impact upon the emergence of a broadly defined and intensely pursued corporate interest as in the case of a larger military establishment. Nor would we expect military values to be as pervasively and deeply inculcated where the armed forces have only been recently created, without a long tradition to give shape and support to these values and without an extensive period within which these values may be instilled and internalized. Where either one or both of these conditions prevail a politicized military will still detract from the possibility of socio-economic change, but their negative impact will be significantly softened. In geographical terms, these two mitigating conditions most frequently obtain in the tropical African countries. Almost half of these 33 countries supported a military establishment of less than 2,000 men as of 1966, and only 6 had a sizeable force numbering more than 10,000 men.[30] And it is in Africa that we find more than two dozen countries that have only recently gained their independence, and thus their military establishments are practically brand new.

III. MILITARY RULE AND MODERNIZATION: CROSS-NATIONAL DATA

In the previous section some reasons were set out for thinking that the officer-politicians' corporate interests and military values dictate a conservative orientation toward economic and social change. By relying upon the cross-national data generated by Adelman and Morris it is possible to test the descriptive generalization that the politicized military do not act as agents of modernization, although the data cannot elucidate the possible validity of the hypothesized motivational explanation based upon the officers' corporate interests and military values. To repeat, Adelman and Morris' data refer to those 74 non-Western, non-communist countries, which have received some kind of foreign assistance from the United States. These countries were placed into three basic categories (with some differentiation within each of them) according to the "political strength of the military."

The first group of countries are those "in which the military was in direct political control during some part of the period 1957–62"; the second group is made up of "countries in which the military was an important political influence but was not in direct political control during most of (this) period;" the third group contains those "countries in which the military had little or no political influence during (this) period."[31] I shall correlate this military dimension of governmental structure with a handful of economic factors that are commonly viewed as indicators of economic and social change. Seven indicators were chosen on the basis of their potential amenability to governmental control or influence. Five of the indicators refer to changes that have taken place as opposed to absolute levels, while the two non-change indicators measure intended and possible future changes.

Over and above their intrinsic weaknesses, all types of aggregate data are bound to have limitations when used for a specific purpose. In this instance I would mention the three most important ones. Half of the economic indicators refer to the period 1950–62, whereas the military variable only covers the period 1957–62. The absence of a complete overlap between the independent and dependent variables is certainly important, its significance only being somewhat mitigated by the fact that many of the countries with a politicized military in the 1957–62 period would fall into a similar category between 1950 and 1957. Secondly, I do not suggest that changes among the economic variables are primarily due to governmental activities; obviously many other factors, from n-achievement attributes to patterns of international trade, have an important bearing upon them. Here I shall assume that these 'extraneous' factors are randomly distributed. Until it is shown that n-achievement, for example, is associated with *both* the rate of GNP growth and military intervention, I am on relatively steady ground in assuming that these 'third' factors do not invalidate the interpretations accorded the correlation coefficients. Thirdly, these indicators imply little about reformist changes. However, according to my previous discussion, if soldiers in mufti do not contribute to economic and social modernization they certainly would not promote social and economic reforms. What follows then is a brief description of the seven modernization indicators.[32]

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[30] David Wood, *The Armed Forces of African States*, Adelphi Paper no. 27 (London, 1966); André Martel, "Les Armées africaines," in Leo Hamon (ed.), *Le Role Extra-Militaire de L'Armee dans le Tiers Monde* (Paris, 1966).

[31] Adelman and Morris, *op. cit.*, pp. 74–76, where the classification of the 74 countries can be found.

The rate of growth of per capita GNP in constant prices between 1950/51 and 1963/64.

Change in the degree of industrialization between 1950 and 1963: This index is based on the average annual change in industrial output in constant prices, the change in the proportion of gross domestic product originating in industry, and the change in the proportion of the total male labor force employed in industry.

Degree of improvement in agricultural productivity since 1950: This measure is based upon the adoption of such improvements as the more extensive use of mechanical power, chemical fertilizers, more modern irrigation systems, better crop rotation, and more scientific breeding.

Rate of improvement in human resources (specifically education) from about 1957 to 1961: This indicator is based upon changes in enrollment at the second level of education as a percentage of the age group fifteen to nineteen, and enrollment at the third level of education as a percentage of the appropriate age group.

Gross investment rate: This measure represents the level of capital formation, with respect to the average ratio of gross investment to gross national product for the period 1957–62.

Change in the effectiveness of tax systems between 1950 and 1963: "The change in the ratio of government domestic revenue to GNP was chosen as one broad measure of the change in over-all success in raising revenue. A second measure of over-all improvement was the average annual rate of increase in real government domestic revenue . . . The third statistical element . . . was the change in the ratio of direct tax to total government revenue, a common indicator of changes in the structure of tax systems."

TABLE 1. THE POLITICAL STRENGTH OF THE MILITARY AND ECONOMIC CHANGE (N = 74)

Rate GNP	Change Industrialization	Change Agricultural Productivity	Expansion Education
.13	.29	.07	.08
Change Tax Level		Investment Level	
.04		−.11	
Leaders' Commitment to Economic Development −.22			

The seventh variable—"leadership commitment to economic development"—stands at a different level of analysis than the others. Since economic change may not have occurred in the 1957–62 period because the government's efforts have not yet yielded the kinds of outputs that are expected in the future, and since leadership commitment may not be translated into economic outputs for any one of a number of reasons (e.g., the bureaucracy is incapable of implementing governmental decisions), this measure serves as a crucial indicator of the governmental incumbents' concern for, and efforts toward, the realization of economic change. In creating their indicator of leadership commitment to economic development for the period 1957–62, Adelman and Morris utilized three types of qualitative judgments: "whether the heads of government and semi-official national agencies (such as ministries of finance, planning agencies, and privately owned central banks) involved in direct or indirect central guidance of the economy typically make concerted efforts to promote economic growth; whether or not this planning includes purposive attempts to alter institutional arrangements that clearly block the achievement of development goals; and whether or not there is a national plan and a planning group functioning within the government which is charged with full time execution of the plan."[33]

A first test of the hypothesis that officer-politicians act as conservatives insofar as they do not contribute to, or block attempts at economic modernization, is found in Table 1, where the political strength of the military is correlated with the economic indicators. It turns out that the mean correlation is .04 and two of the correlations are negative. The only indication to the contrary is the .29 correlation with the pace of industrialization. Yet even this single piece of evidence is accounted for by the reasoning underlying the hypothesis itself. For, as was previously noted, the one type of economic change that the military might undertake because of its corporate interests is industrial growth. The data in Table 1 therefore suggest the following three statements, although they cannot be accepted until various counter-arguments and interpretations are first evaluated.[34] (1) If the military do act as powerful agents of economic change in the non-Western countries then we should have found strong positive correlations here. Thier absence seriously calls into question the arguments and conclusions of Shils, Halpern, Pye and Pauker. (2) The zero order correlations in Table 1 suggest that the officer-politicians do not make any efforts or contributions to economic change, although it is necessary to control for possible "third" factors before this generalization can be asserted with any degree of confidence. (3) On the safe assumption that political organizations, groups or strata are actively demanding and attempting to bring about economic change in many military regimes, the officer-politicians oppose them. This generalization does not emerge solely from the data presented in Table 1. Its further demonstration will have to wait until part IV of this essay where it will be seen that in those countries characterized by a significant level of demands, organization and participation directed toward the realization of economic change, there is no positive relationship between the military's political strength and economic change. This finding therefore implies that the soldiers in mufti oppose such groups and strata in their quest for economic change.

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[32] *Ibid.*, pp. 84–129 and *passim;* also see pp. 14–15 for the assignment of numerical scores.

[33] *Ibid.*, pp. 78–81. While economic planning might be thought to discriminate against military regimes, Janowitz (*op. cit.*, pp. 64–65) and others have emphasized the officers' statist predispositions.

[34] Further confirmation of the absence of any positive relationship between the military's political strength and economic change is evidenced in the following three operations. (1) As an additional

The first basic objection that could be raised concerning the interpretation of the data concerns the presumed direction of the causal arrow. It might be thought that the arrow should lead from the economic variables to the political-military one, rather than vice versa. Certainly most attempts to relate economic and political phenomena have pursued this approach. However, I do not think that this is a valid criticism of the present interpretation. While it is more than possible for economic change or its absence to influence the degree and frequency of military intervention, the zero order correlations in Table 1 indicate that these particular economic variables do not help account for the political strength of the military, at least during overlapping time periods. It could be that low or exceptionally high levels of economic change in the period prior to 1957 (i.e., at time t minus 5) contributed to the military's acquisition or maintenance of political power in the 1957–62 period. But whether this time-lagged relationship exists or not is quite irrelevant to the primary objective of the present essay which centers about the consequences of military rule *after* the officers have entered the political arena.

Secondly, it might appear warranted to argue that the zero order correlations fail to disprove the hypothesis that the military act as agents of modernization when they enjoy substantial political influence or control. For any modernizing efforts and programs would probably not have much impact upon economic change until some years after their implementation. This point is well taken, especially since we are not differentiating between those countries in which the military did or did not exercise political influence prior to 1957, nor do we possess comparable economic indicators for the 1962–67 period (i.e., at time t plus 5). If economic change were to occur at time t plus 5 the government would have to have made efforts in that direction and invested in the economy during the period 1957–62 (i.e., at time t_1). Yet during this period the correlations between the military's political strength and the governmental leaders' commitment to economic development is −.22, while the correlation with the gross investment rate is −.11. Given these data it is quite unlikely that countries with a politicized military in the 1957–62 period will have experienced economic change between 1962 and 1967. Furthermore, in constructing the political strength of the military index, Adelman and Morris explicitly incorporated the time factor into their classification scheme. Their scale is ordered from high to low not only according to the political strength of the military—whether they were dominant, influential or neutral—but also according to the length of time that the officer-politicians were in

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test for the reliability of the mean correlation of .04, I correlated a single indicator of economic modernization (which was constructed by calculating the mean score for each country on the seven economic indicators) with the military variable. The result was a correlation of .08, which hardly differs from the .04 mean correlation in Table 1. (2) To test for the possible effects of curvilinear patterns and extreme scores, each of the seven indicators was separately cross-tabulated with the military variable. There were no significant deviations from statistical independence, except for a weak positive relationship between the military variable and change in industrialization (gamma = +.38) and leadership commitment to economic development (gamma = −.28)—which is what we would expect given the .29 and −.22 correlations in Table 1. (3) On the possibility that Adelman and Morris' distinction between the first and second groups of countries classified according to the military's political influence (those in which the military enjoyed a "marked" and "moderate" political role) is far smaller in actuality than the assumed equal difference between the second and third groups of countries ("moderate" and "negligible" political influence), I collapsed the first two categories and performed the above tests without any differences in the resulting measures of association.

direct control of the government or exerting influence over the civilian incumbents. Within each of the two categories of the military's political involvement, those countries in which the officers were politically influential for five years were scored higher than those in which they were influential for three years, and the latter received higher scores than countries in which the military only exercised their influence for one year.[35] According to the counter-argument that the military's modernizing efforts will not immediately have their intended impact, we would expect to find that within each of the two groups of countries experiencing military rule, those countries in which the military exercised their political influence for a longer time period would be characterized by greater economic change than countries in which the military were politically involved for only a short period of time. Yet an examination of the scatter plots for the two groups of military dominated countries does not reveal any linear increase in rates of economic change when the length of involvement is considered.

Thirdly, it is necessary to test for possible spuriousness in the correlations before the data can be accepted. Do the zero order correlations in Table 1 survive or change when third factors are introduced? In particular, what happens when bureaucratic proficiency, political stability and level of economic development are taken into account? The data in Table 1 might be due to an association between military rule and a paucity of economic and administrative skills among the civil servants responsible for advising the officer-politicians on economic matters and administering the government's economic programs. In the absence of a well organized and competent bureaucracy any government would have great difficulty in bringing about economic change.[36] Adelman and Morris constructed an index of administrative efficiency for the 1957–62 period based upon three qualitative criteria: "The degree of permanence and training of administrators (an indirect measure of whether recruitment is based upon qualifications for the job); the extent to which corruption, inefficiency, and incompetence seriously hamper governmental functioning; and the extent to which instability of policy at higher levels of administration promotes inefficiency."[37] With the exception of the last criterion, this index provides a reasonably valid measure of the bureaucracy's competence to deal with economic change if expected to do so by the governmental incumbents. The 74 countries were then stratified into three groups according to their level of administrative efficiency. If the zero order correlations were spurious, positive correlations between the military in politics and the economic indicators should have appeared among those countries that enjoyed a high level of bureaucratic competence. However, this turned out not to be the case, thereby pointing to the officers' motivations as the explanation for the absence of any significant correlations in Table 1.

Another possible counter-argument would attribute the low correlations in Table 1 to the circumstances under which the military usually come to exert their political strength. The military are often propelled into the political arena, and often remain there, when their countries are in the throes of violence and governmental instability. Since there is a sizeable negative association between political stability[38] and the political strength of the military ($r = -.44$), and between political stability and an index of economic modernization based on the seven indicators ($r = -.35$), it may be that the officers' do have modernizing goals but they cannot be implemented due to the prevailing instability. While this reasoning seems eminently sensible, the data do not bear it out. When the 74 countries are stratified into four groups according to the level of political stability, and the political strength of the military is then correlated with the economic variables, we should find strong positive correlations among the more stable countries. Yet this turns out not to be the case at all; there is no apparent difference between the military's success in promoting economic change within more or less stable political contexts.

Lastly, a counter-argument might be found in the statement that *any* government would find it extremely difficult, perhaps even impossible, to bring about significant economic change in the very poorest countries that lack even a minimal economic infrastructure. We should therefore expect the correlations between the military's political strength and our economic indicators to be higher in the relatively wealthy non-western countries than in the exceptionally poor ones, the low correlations among the latter being no fault of the military's. However, when the sample is stratified into four groups according to their GNP levels, the correlations are no higher in the high GNP countries than in the low ones. In fact, in the wealthiest countries the correlations tend to be slightly negative, while in the very poorest countries there are significant positive correlations between the politicized military and economic modernization. This last finding serves as a clue to the explanatory hypothesis developed in the next section.

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[35] Adelman and Morris, *op. cit.*, p. 75.

[36] A separate analysis of the Adelman and Morris data indicates that the nature of the bureaucracy has an important bearing upon economic change where governments are attempting to modernize their economies.

[37] *Ibid.*, pp. 78–81.

[38] Adelman and Morris' political stability index is based upon (1) the extensiveness of internal security, or in reverse fashion, the level of violence, (2) the extent of continuity in the form of government (which partly overlaps with the political strength of the military), and (3) the extent of overt consensus regarding the prevailing form of government. (*Op. cit.*, pp. 81–83.)

IV. THE MIDDLE CLASS ORIENTATIONS OF OFFICER-POLITICIANS

The day has passed when officers were almost exclusively recruited from the aristocracy and the traditional land-owning class. Today military officers are commonly drawn from the middle class, the marginal middle class, small landholders, and not infrequently from rural and (in Latin America) urban laborers.[39] Where the officers do not originate in the solid middle class they are at one with their fellow officers in having achieved a solid middle class occupational and status rank. As members of the middle class by birth and/or achievement, soldiers in mufti act in accordance with their class' interests and identify with its civilian members.[40] Pre-adult socialization experiences, family connections by birth or marriage, social contacts, status aspirations, property ownership, business connections, political relationships—these factors bind the officers to the middle class' interests and lead them to identify with its values and members. Consequently, a politicized and largely autonomous officer corps would be unwilling to undertake or sanction policies of change that may endanger the middle class' economic perquisites and political prerogatives.[41] (Parenthetically, in my estimate class interest and identity is not as compelling a motivation as is the military's corporate interests and values.)[42]

This hypothesis and the contrary one put forward by Halpern, Pauker, Johnson and others begin with the same premise: the great majority of officers come from middle and lower middle class backgrounds, and there is even some recruitment from lower social strata. I agree with these writers when they argue that this kind of background, and the officers' adult middle class status, turns them into opponents of aristocratic, feudal and land-owning strata. But this statement does not allow us to conclude that officer-politicians are concerned with socio-economic change, and certainly not with "progressive" change. To oppose the upper classes is not necessarily to support the lower classes, especially in the non-western zero-sum political arena and in the context of economic scarcity, although there have been some temporary alliances between middle class officers and the lower classes. Moreover, the proponents of the reverse hypothesis are attributing an inordinate importance to the officers' backgrounds—the disadvantages, marginality, degradation, and poverty many of them experienced before entering the military. These experiences often provide the impetus for individual mobility, but they need not instill a broader concern for the plight of

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[39] Janowitz, *op. cit.*, pp. 49–58; also see the references to Shils, Johnson, McAlister, Pauker and Halpern above.

[40] And this despite C. Wright Mills' assertion that "social origins and early background are less important to the character of the professional military man than to any other high social type," in *The Power Elite* (New York, 1952), p. 192. Although this generalization has a good deal of validity, the present study centers upon those officers that are acting outside of their instrumental or professional role. And Janowitz has noted that the officers' social origins are politically more salient in contemporary non-western countries than in western societies. (*op. cit.*, p. 56)

[41] Until recently, the prevailing view among students of Latin American politics contradicted this assertion. It was thought that the middle class and the military were mutually antagonistic, the military blocking the middle class' quest for economic modernization and democratic government. However, since José Nun pointed to the frequency of "the middle class military coup," the opposite viewpoint has gained a respectable measure of support: José Nun, "The Middle Class Military Coup," in Claudio Veliz (ed.), *The Politics of Conformity in Latin America* (New York, 1967); Charles Wagley, *The Latin American Tradition* (New York, 1968), pp. 203–212.

[42] The greater salience of corporate interests compared to class interests is sharply illustrated in the case of the 1963 Ecuadorian *coup*. The rationale for the take-over was a thorough-going anti-communism. But the officers' anti-communism was not primarily the product of their middle class interests: it was based upon the protection of their corporate interests. For the officers believed that if the communists came to power they would replace the army with a militia. See Martin C. Needler, *Anatomy of a Coup d'Etat: Ecuador 1963* (Washington, 1964).

TABLE 2. THE POLITICAL STRENGTH OF THE MILITARY AND ECONOMIC CHANGE ACCORDING TO SIZE OF THE MIDDLE CLASS

	Rate GNP	Change industrialization	Change agricultural productivity	Expansion education
Medium sized middle class (N=12)	−.17	.05	−.12	.12
Small middle class (N=33)	−.06	.17	.04	−.18
Miniscule middle class (N=29)	.34	.44	.17	.34

	Change tax level	Investment level
Middle sized middle class	.01	.19
Small middle class	−.07	−.48
Miniscule middle class	.09	.07

	Leaders' commitment to economic development
Medium sized middle class	.17
Small middle class	−.02
Miniscule middle class	−.04

less fortunate strata. In fact, where individual aspirations have been realized there is generally a reduced concern for collective mobility. Undoubtedly the iniquitous and inequitous experiences of some officers were sufficiently poignant and their memories vivid enough to maintain a concern for *los de abajo* throughout their careers. But despite the importance of socializing experiences we cannot assume that they are generally more salient than adult class and status positions. In fact, where there is an incongruity between the past and present there is good reason to assume that the latter is more salient in shaping behavior—at least until proved differently. Although the Marxian hypothesis is obviously not universally valid, Marx was more often right then wrong, and therefore it makes sense to begin with it as a working hypothesis.

Having put forward a generalization regarding the influence of the officers' middle class interests and identities as these shape their governmental decisions, I must now add an important modification, although one that follows logically from the hypothesis. We would expect soldiers in mufti to act differently according to variations in the social structure and the distribution of political power; where modernizing changes and mass political participation are not perceived as threats to the middle class' material and political privileges we would not necessarily expect the military to oppose them, and other factors may even lead the officers to allow such changes. Soldiers in mufti will protect the status quo only where the middle class' interests are seen to be threatened. When the non-western countries are stratified according to their class structure and the distribution of political power we would therefore expect the politicized military to oppose economic change at one end of the continua, perhaps allowing change to occur at the other end, although the hypothesis does not allow us to predict the critical points on the continua where the military's class-defined interests and identities may undergo a marked change.[43]

In Table 2 the sample is divided into three groups according to the size of the indigenous middle class, i.e., the proportion of the active male population in "commerce, banking, insurance, or in technical, professional, managerial, administrative or clerical employments." The three groups refer to those countries in which at least 20% of the active male population was engaged in such occupations, in which between 10 and 19% held such jobs, and in which less than 10% did so.[44] When the military's political influence is correlated with our economic indicators in these different class contexts we find that they fail to sponsor economic change when there is a medium sized or even a small middle class. Without exception the correlations are either weakly positive or negative. Apparently once a middle class constituting over 10% of the active male population emerges, officers in mufti act to conserve its perquisites by opting for the status quo. Only in the context of a miniscule middle class do the officer-politicians allow for some economic change. Among these countries the military's political strength is positively correlated with growth in GNP (.34), with industrialization (.44), and with expansion of education (.34).

The data in Table 2 thus tend to bear out the hypothesis that the officers act in accordance with their class interests, either failing to act as modernizing agents or opposing economic and social change where the middle class is relatively wealthy and established—where change is presumably seen to involve the redistribution of economic privileges. But how do the military's class interests account for their mild sponsorship of economic change in countries with a miniscule middle class? As leading members of a class that is not nearly as well off as its foreign equivalents, and therefore anxious to secure greater wealth and prestige for the class as a whole and thus for themselves, the officer-politicians are willing to permit economic change to occur because it is part of the modernization process whose culmination is seen in terms of a sizeable and economically established middle class. In addition, the officers' class interests are not especially threatened by the lower classes; for where only a miniscule middle class exists it is improbable that a significant number of peasants and workers have been brought into strategic economic and political positions through urbanization, commercialization and industrialization. On the contrary, any threat to the officers' class interests would be found among those defenders of the traditional order, thereby turning soldiers in mufti into reformers and agents of change.

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[43] With respect to Latin America, a contrary generalization is found in Johnson, *The Military and Society . . .*, *op. cit.*, pp. 137–138.

[44] Adelman and Morris, *op. cit.*, pp. 30–33.

The class hypothesis derives additional support from a comparison of the economic variables which do and do not correlate with military involvement where there is a miniscule middle class. Although alternative interpretations are indeed possible, the ones I place upon the data are at least plausible. In predominantly agricultural economies (which is what we are dealing with in the presence of a miniscule middle class) GNP may be increased through industrial growth, increased agricultural productivity, or both. If the officer-politicians were acting in accordance with their middle class interests and identities we would expect them to emphasize industrial growth[45] at the expense of agricultural production, for the former has a far greater impact upon the nature and size of the urban middle class than does the latter. And it turns out that there is a positive correlation with industrial growth (.44), and a practically negligible correlation with increased agricultural productivity (.17). There is an intimate association between middle class occupations and post-elementary education, which may account for the .34 correlation between military involvement and educational expansion. On the other hand, there is a zero-order correlation (.09) with changes in the tax level, and such changes would hurt the existing middle class. Not only does this indicator measure the amount of tax monies collected, it also gets at the change from regressive indirect taxes which benefit the middle class to nonregressive direct taxes. Then there is the contrast between the −.04 correlation with the governmental leaders' commitment to economic development and the .34 correlation with rate of GNP growth. This would suggest that economic change is due to the officer-politicians' permissive orientation toward change rather than any sustained efforts on their part to bring it about. And this makes good sense according to the class interpretation: economic change is in accord with the interests and prestige seeking identities of the middle class officers, but since they themselves have already achieved many of the perquisites of middle class life there is not sufficient motivation for them to undertake strenuous programs of economic modernization.

This middle class' perception of their "stake in society" is shaped by the distribution of political power as well as the class structure. Where that stake is thought to be endangered by the acquisition of power by peasants, workers and disadvantaged ethnic groups demanding governmental responsiveness to their economic aspirations, the officers act as conservatives; where this threat from below has not yet gathered strength the officers allow for economic change. There is a great overlap between this hypothesis and the central dimension of Huntington's outstanding analysis of the officers' political behavior—"The extent to which a politicized officer corps plays a conservative or a reform role in politics is a function of the expansion of political participation"—and thus the data presented here tend to bear out his generalization. In oligarchical societies the soldier is a radical; in societies dominated by the middle class the officers act as arbitrators among middle class groups; and when mass political participation is in sight the soldier protects the existing order.[46] There is only one revision that ought to be made of Huntington's interpretation. Analysis of the Adelman and Morris data indicates that it is only at the very lowest levels of political participation and only in the context of a miniscule middle class that the officers sponsor modernizing policies, whereas Huntington is somewhat more generous in estimating the parameters within which this occurs. For example, contrary to his acceptance of Halpern's thesis with respect to the Middle Eastern countries because of their low level of political participation,[47] the data to be presented

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[45] Admittedly I have already imputed the officers' concern with industrial growth to their corporate interests, thereby possibly over-determining this variable.

[46] Huntington, *op. cit.*, pp. 221–222 and *passim*. As it applies to Latin America, the same argument is found in Nun, *op. cit.*, esp. p. 103.

[47] Huntington, *op. cit.*, p. 219.

TABLE 3. THE POLITICAL STRENGTH OF THE MILITARY AND ECONOMIC CHANGE ACCORDING TO THE LEVEL OF ECONOMIC DEMANDS

	Rate GNP	Change industrialization	Change agricultural productivity	Expansion education
Urban and rural demand countries (N = 21)	−.20	.24	−.11	−.20
Urban demand countries (N = 21)	.22	.16	−.02	.05
No demand countries (N = 32)	.39	.59	.45	.53

	Change Tax Level	Investment Level
Urban and rural demand countries	.13	−.09
Urban demand countries	−.27	−.45
No demand countries	.34	.19

	Leaders' commitment to economic development
Urban and rural demand countries	−.09
Urban demand countries	−.30
No demand countries	−.22

below indicate that the officers are not agents of modernization in that area of the world.

A crude test of this hypothesis is possible by relying upon Adelman and Morris' "modernization of outlook index" insofar as it provides a rough and ready measure of the extensiveness of economic demands and the degree to which these demands are backed up by political participation and organizational strength. The index was constructed in the following manner: First, we divided countries into two broad classes: those in which the outlook of educated urban groups was 'significantly' modernized and those in which it was not. Countries were judged to have significant modernization of outlook only if the adoption of Western or modern styles of living had gone considerably farther than external forms and dress, and had entailed the evolution of at least some important modern forms of social and political participation such as voluntary associations. The second element in the index was a judgment of whether programs of political, social, and economic modernization (health programs, economic institution-building) have or have not gained significant support among the urban population and among some of the rural population.[48] In Table 3 the sample is stratified into three groups. The first includes those countries "in which the outlook of the urban educated population was significantly modernized, judging by the standards of less-developed countries, and in which programs of political, social and economic modernization had gained significant support among both the urban and rural populations." The second group has the same characteristics as the first except that the rural population is not demanding modernizing policies. The third group contains "countries in which the outlook of the educated urban sector was partially but not significantly modernized and in which programs of modernization, if they existed, had gained relatively little support among either the urban or rural population."[49]

In the first two groups of countries economic aspirations are widely articulated and supported by a measure of political participation. The correlations between the military's political strength and the economic variables can therefore be interpreted as indications of the officer-politicians' inclinations toward economic change in countries characterized by a significant level of economic demands. The correlations point to the absence of any positive inclination, and thus a lack of popular responsiveness as well. More than half of the correlations are negative and the mean correlations among these "demand" countries is −.05. The politically influential military apparently act as conservatives, opposing those organizations, groups and strata that are working for economic change, presumably because fairly widespread economic demands constitute a threat to the prevailing distribution of power that is politically and financially beneficial to the middle class. These data may also be interpreted in terms of the officers' corporate interests and military values; a politicized working class and peasantry are seen as threats to the military establishment itself and as harbingers of a disorderly politics, obviating any responsiveness to their demands.[50]

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[48] Adelman and Morris, *op. cit.*, p. 50. Although there is a high correlation between this index and the size of the middle class (r = .72), the two variables are analytically and empirically distinct and thus they should be analyzed separately.

[49] *Ibid.*

[50] On a more speculative level, it may be that the threat from below engenders a sense of political

TABLE 4. POLITICAL STRENGTH OF THE MILITARY AND ECONOMIC CHANGE IN FOUR GEOGRAPHICAL REGIONS

	Latin America (N = 21)	Middle East and North Africa (N = 15)	Asia (N = 15)	Tropical Africa (N = 23)
Mean size of the middle class	71	62	51	37
Mean level of economic demands	66	47	37	27
Rate GNP	.01	−.28	.03	.45
Change industrialization	.16	.03	−.02	.42
Change agricultural productivity	−.06	−.03	−.39	.60
Expansion education	−.43	−.12	−.31	.34
Change tax level	−.14	−.11	−.07	.07
Investment level	−.38	−.32	−.26	.06
Leaders' commitment to economic development	−.43	−.16	−.17	.08
Mean correlation	−.18	−.14	−.17	.29

In contrast, all but one of the correlations are positive among the "no demand" countries, where the lower classes are politically quiescent and acquiescent. The degree to which the officer corps is politicized correlates with the economic indicators at the following levels: rate of GNP increase .39, change in the level of industrialization .59, change in agricultural productivity .45, and expansion of educational enrollments .53. In this group of countries the disadvantaged strata represent a preciously small threat to the middle class *or* the military establishment. This duality would account for the stronger correlations among the "no demand" countries than among countries with a miniscule middle class. The officers can therefore allow economic change to take place without feeling threatened by, or hostile toward the lower classes, and they may even sponsor those changes which foster the emergence of a sizeable and prosperous middle class. There is also the possibility—although that is all it is—that the officer-politicians occasionally satisfy some of the lower classes economic desires (as opposed to demands) in order to win their support for modernization programs that challenge the interests and positions of traditional forces, whether these be tribal chiefs, aristocrats, monarchs or large land holders.

Given the apparent plausibility of the class-oriented hypothesis and its corollary, it would be useful to stratify the sample geographically. We might then gain a better general understanding of the politics of particular geographical areas by gauging the variations in the politicized military's activities as modernizing agents; a geographical stratification would identify those areas in which the miniscule middle class and a practically non-existent demand level provide the officers with permissive or even positive orientations toward economic change. In addition, given the fact that statements regarding the purported modernizing consequences of military rule have usually been generalized from a particular area of the world, and the not uncommon belief that it is only in Latin America that soldiers in mufti act as defenders of the existing order, a geographical stratification of the sample would help clarify the issue.

A glance at Table 4 indicates that is is not only in Latin America, but in Asia and the Middle East and North Africa as well, that officer-politicians either fail to contribute to economic change or oppose modernizing demands and efforts where these exist. The mean correla-

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insecurity among soldiers in mufti, which would further decrease their readiness to fulfill the lower classes' economic demands. Social psychologists are well aware of the aggressiveness, hostility and distrust that grows out of feelings of insecurity. And these reactions could only provide the officer politicians with an added impetus to ignore or repress popular demands. This hypothesis would be particularly applicable where the demands are of a novel type or especially wide-ranging in their impact, such as demands for land reform. Whereas politically secure politicians may or may not be capable of departing from past practices and accepting innovative changes, insecure politicians are likely to reject or repress them due to an anxiety-dictated rigidity. Robert E. Scott emphasizes the insecurities found among Latin American elites due to continuing economic and political change. See his "Political Elites and Political Modernization: The Crisis of Transition," in Seymour Martin Lipset and Aldo Solari (eds.), *Elites in Latin America* (New York, 1967), pp. 117–127 and *passim*.

tions in these three geographical regions are slightly on the negative side, ranging from −.14 to −.18. There are sharp variations with respect to three economic variables, but in each case it is a different region that evidences the strongest negative correlation. In Latin America leadership commitment to economic development correlates at the −.43 level with the military's political strength, decreasing to −.16 and −.17 in the two other regions. In the Middle East and North Africa there is a −.28 correlation with the rate GNP growth, with .01 and .03 correlations in the other two regions. In Asia change in agricultural productivity is associated with the military at the −.39 level, while in the other regions the correlations are −.06 and −.03. Even though there are significant variations in the mean levels of economic demands and the size of the middle class among these three regions, there is little corresponding variation in the officer-politicians' motivations. Demands are presumably already too extensive and the middle class sufficiently established to obviate the politicized military giving their positive support, encouragement or approval to modernization policies.

The tropical African countries register the lowest mean level of demands and the smallest middle classes (approximately half of the Latin American averages), which presumably helps account for the positive correlations between the military's political strength and the rate of GNP increase (.45), industrial growth (.42), increased agricultural productivity (.60), and educational expansion (.34).[51] But even here the military allow for economic change rather than actively and purposefully facilitating it, for the correlation with leadership commitment to economic development is .08. There is another factor which is relevant to the interpretation of these positive correlations. I previously pointed out that the officers' corporate interests are pursued less determinedly and their military values are less deeply instilled where the armed forces are especially small and the military establishment is quite new. Of the four geographical areas these conditions clearly obtain most frequently in Africa, which means that the African officers' corporate interests and military values only moderately detract from their modernizing inclinations.

Even a cursory examination of the non-western countries during the last 30 or 40 years would immediately highlight the growth of the middle class and the expansion of political participation in most of them; and since the class-oriented hypothesis is based upon these two factors it should be applicable longitudinally as well as cross-nationally. We would expect the consequences of military rule to vary according to alterations in a country's social and political contours over time. Among non-western countries such changes began earliest among many of the Latin American states. In the 1920's and 1930's a small professional, entrepreneurial and commercial middle class emerged, followed shortly by the growth of an urban working class. The appearance of the middle class placed a new strata between the large landholders and an illiterate rural labor force, while the growth and urban concentration of the working class enhanced the lower classes' political power. In the 1930's it was not uncommon for the middle class and the urban laborers to join together in attacking the political bastions of the traditional land-holding class. After World War II the more common outcome was class conflict between these former allies. And it was, of course, during the 1950's and 1960's that the urban working class, and the agricultural workers to a much lesser degree, became sufficiently mobilized to become serious contenders for governmental power. Thus according to the hypothesis we would expect Latin America's officer-politicians to have become increasingly conservative since the inter-war period.

Martin C. Needler's study of Latin American politics contains a classification of the successful *coups* that occurred over the last thirty years. Unfortunately Needler does not provide us with the criteria he used in classifying these *coups* as "reformist." Not knowing precisely what Needler is measuring we are unable to judge the extent to which his data may be used as evidence for our proposition. However, on the reasonable assumption that "reformist" refers at least in large part to the goals of economic change and/or the mitigation of poverty and glaring inequalities of opportunity, then his data do support the hypothesis. For the number of "reformist" *coups* as a proportion of successful *coups* decreases steadily since 1935: between 1935 and 1944, 50% of the successful *coups* are classified as "reformist," dropping to 23% in the 1945–54 period, and falling further to 17% between 1955 and 1964.[52] This decreasing pattern is probably related to the increasing size of the Latin American middle classes since 1935. When that class was quite small and the entrenched land-owning class dominated both economy and polity, the officers acted out their middle class interests and identities by challenging the upper class' economic priviledges while facilitating the expansion of the middle class through economic change. As the middle class grew in size, wealth and power, the challenge to its perquisites came from the less advantaged strata, thereby sharply reducing the officers' modernizing activities and turning them into protectors of the status quo. And this despite the widely believed reformist lesson found in Castro's revolution. Needler's classification also shows that the proportion of *coups* that occurred just before or right after national elections increased from 12%, to 32%, to 56% during the three decades from 1935 to 1965.[53] When viewed alongside the decline in the proportion of "reformist" *coups*, these figures imply that as the lower classes became politically mobilized, the military increasingly stepped in to prevent or abrogate the results of those elections which registered a political threat to the middle class' political predominance and economic interests.

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[51] There is some reason for thinking that the correlation coefficients for the African countries are somewhat artificial. The military were politically influential in only 6 of the 23 African regimes during the 1957–62 period, and in only one other did officers occupy the seats of government. And an examination of the scatter plots shows that the positive correlations are largely the product of 14 "low-low" cases, civilian regimes doing poorly economically, rather than officer-politicians doing well.

Lastly, I should make an attempt to deal with those deviant cases in which the officer-politicians did subscribe to the tenets of modernization and reform in the context of a sizeable middle class and a partially mobilized lower class. Every non-tautological generalization that is specific enough to be testable will encounter at least a large handful of deviant cases. But if the proposition itself can help interpret some of these deviant cases its plausibility is concomitantly heightened. I would begin by noting that in practically all those instances in which the military intervened with serious intentions regarding modernization and reform, it was the younger captains and colonels that led the *coups*. For example, the Egyptian, Iraqi and Turkish juntas formed in 1952, 1958 and 1960 respectively, consisted of officers with the rank of colonel or below; it was they who planned the coups and then co-opted older generals to serve as respected figureheads. And in each instance the general turned out to be more conservative than his juniors.[54] Throughout the Middle East the senior officers have been politically more conservative and more strongly attached to the social status quo than the junior officers.[55] "Almost invariably, Latin America's popular revolutions of this century were led by young officers. They became the sponsors of fundamental change and reform, the underminers of traditional institutions, the proponents of public-welfare measures."[56]

How can the hypothesis that soldiers in mufti act in accordance with their middle class interests and identities help account for this pattern? As captains, majors and colonels they are members of the middle class, but not only is their income incommensurate with their rank when compared to the incomes of the senior officers, promotions come slowly at this level, and only a proportion of the middle level officers are slated to enjoy the generals' class and status perquisites. The generals have reached the topmost rungs of the middle class pyramid; they are contented compared to the captains and colonels, many of whom are attempting to further fulfill their middle class aspirations. In countries whose political institutions invite military intervention this becomes a broad avenue open to these aspiring middle level officers. But to carry out a *coup* which will propel them into the highest rungs of the middle class pyramid requires a rationale or legitimizing banner that the generals are unwilling to grasp. By stating their cause in terms of modernization and reform the middle level officers can oppose and then replace their conservative superiors. Once having installed themselves in power some of these young officers will act in accordance with their original goals. On the other hand, this reasoning also suggests why so many of the young officers who apparently begin their political adventures as ardent reformers become conservatives only a few short months after their *coups*. They were not convinced reformers to begin with; they simply used the call for economic and social change for the realization of their own class and status interests.

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[52] Martin C. Needler, *Political Development in Latin America: Instability, Violence and Evolutionary Change* (New York, 1968), p. 65.

[53] *Ibid.*

[54] Dankwart A. Rustow, "The Military in Middle Eastern Society and Politics," in Sydney Nettleton Fisher (ed.), *The Military in the Middle East* (Columbus, 1963), p. 15.

[55] James A. Bill, "The Military and Modernization in the Middle East," *Comparative Politics,* 2 (October 1969), 56–57.

[56] Lieuwen, *Arms and Politics . . . , op. cit.*, p. 126.

THE REPRESENTATION OF CITIZENS BY POLITICAL AUTHORITIES: CONSEQUENCES FOR REGIME SUPPORT*

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Representation is a matter of linkage. In this paper it is argued that a useful handle can be gained on the problem of identifying and measuring representational relationships empirically by viewing representation as a type of support linkage between members of political systems and the authorities.[1]

To conceive representation as a type of support linkage is to direct attention primarily to the represented rather than the representative. Representational relationships have functional significance for political systems particularly because they are linkages which involve members' satisfaction-dissatisfaction with the behavior of the political authorities—linkages which reflect the degree to which members feel that the performance of the authorities "stands for" or "re-presents" their own interests; and this performance satisfaction-dissatisfaction presumably makes a contribution to more general support for the political system. In contrast to legitimacy sentiments, which are independent of immediate outputs from political authorities, members' perceptions of representational linkages between themselves and the authorities depend on their affective responses to outputs, encompassing not only instrumental performance satisfactions, but (and most commonly among the membership in general) symbolic performance satisfactions as well.[2]

This paper reports an exploratory investigation of a construct for measuring sense of representation. The sample consists of a group of students enrolled at the University of Iowa—including, in order to ensure adequate variation on the support scales, a number of students arrested for participating in a protest demonstration against the presence of Marine recruiters on the University of Iowa campus.[3]

The research is concerned with three questions: (1) Is the proposed sense-of-representation construct a valid measure of this phenomenon? (2) To what extent are an individual's sense of representation by Congressman, Congress, the Supreme Court, and President (Johnson) interrelated and associated with the ideological, structural, and personal dimensions of legitimacy? (3) What is the impact of sense of representation on the disposition to extend or withdraw behavioral support?

I. REPRESENTATION AND SUPPORT

In one of the more suggestive analyses of representation to date, Wahlke documents the ways in which traditional demand-response conceptions of representation are deficient, and argues that representation may be more fruitfully studied by focusing on manifestations of support for political objects from citizens.[4] According to demand-response conceptions, representation is seen to inhere in the relationship between members' policy demands and the policy-making activities of political authorities. In the case of representational linkages between citizens and legislators, empirical studies largely have taken it for granted that "the ultimate test of 'representativeness,' whether by Burkean 'trustee' or 'delegate' legislators . . . or whatever legislative role [is examined], is still . . .

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* Financial Assistance for this study was provided by the Graduate College of the University of Iowa. An earlier version of this paper was presented at the 65th Annual Meeting of the American Political Science Association, September, 1969.

[1] This approach to the empirical study of representation has been suggested by John Wahlke in his "Public Policy and Representative Government: The Role of the Represented," a paper prepared for the Seventh World Congress of the International Political Science Association, Brussels, Belgium, September, 1967, and published as Report #9 from the Laboratory for Political Research, University of Iowa, 1967.

[2] Symbolic or expressive satisfactions are discussed at length in Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1964).

[3] This sample is also discussed in an earlier report by the author, "Correlates and Consequences of Beliefs in the Legitimacy of Regime Structures," *Midwest Journal of Political Science* (August, 1970).

[4] Wahlke, "Public Policy and Representative Government . . ."

the match between legislative policy output and public or constituency policy wishes."[5] However, much public opinion research, which shows that most citizens do not entertain clearly defined policy preferences, challenges the notion that congruence between citizen policy preferences and legislative policy decisions is the crucial representational linkage.[6]

To look for representation in support linkages between citizens and political authorities is not to ignore congruence between policy demands and policy outputs. Among attentive citizens, policy satisfactions may be important components of representational relationships.[7] However, conceiving representation in terms of support does draw attention away from exclusive focus on demand-response congruence, and thereby suggests consideration of linkages other than the demand-response variety, linkages that may involve primarily expressive rather than instrumental satisfactions, and may thus be far more salient for the membership in general. Even more importantly, as Wahlke has pointed out, "it is the role of the represented, even more than that of the representative . . . which is highlighted by focusing on the problem of support."[8] Regardless of whether the representational relationship involves instrumental or expressive satisfactions, focusing on support underlines the importance of citizens' reactions to the behavior of political authorities—of representational relationships actually being *perceived* by citizens. For, although demand-response congruence can be estimated without ever taking into account whether such congruence or lack of congruence is actually perceived by the represented, or the degree of its salience for him,[9] conceiving representation as a type of support necessarily brings *perceptions* of congruence (of course, not only of the demand-response variety) to the center of the stage, where they should be, if interest is in the consequences of representational relationships for the persistence and maintenance of political systems.[10]

Finally, considering representation in terms of support also calls attention to the fact that legislative bodies are not the only loci of representational linkages in political systems. Of course, this has not gone unrecognized by scholars; in the American system, the presidency is a representative institution of unquestionable importance, and, as Roger Davidson has emphasized, "the non-elective federal bureaucracy also has significant and long-standing claims as a representative instrument."[11] But demand-response conceptions have tended to focus singularly on legislative institutions, where the responsiveness of representatives to the demands of the represented is presumably maximized by the electoral process.

The approach to be followed here is certainly not the only way of conceiving of representation. What is or is not a useful conception depends on one's theoretical objectives. The approach suggested recently by Prewitt and Eulau may be useful if the theoretical objective is that of investigating "the processes available to the public for controlling and holding accountable the political leadership . . ."[12] For their purposes, Prewitt and Eulau apply the notion of the representative's decisional style vis-a-vis constituency policy demands, originally examined with respect to individual legislators by Eulau et al., to *groups* of legislators.[13] Their concern is with the composite representational response style of city councils to constituency policy demands. Representational response style is treated as a collective property: councils in which the individual members emphasize that their council is responsive to attentive publics, and those in which the individual members emphasize that their council is responsive to ad hoc issue groups, can be said to manifest collective types of the delegate representational style; councils in which the members emphasize that their council entertains "a self-defined image of what community needs are," can be said to collectively manifest the trustee representational style.[14] Prewitt and Eulau adopt a demand-response conception of representation, according to which only those councils that respond to citizen policy demands are considered to be representative bodies: "representation is taken to mean a relationship between governed and governors wherein the governing group responds to ('represents') politically organized viewpoints among citizens, that is, responds to something *other* than its own image of what the community needs."[15] Although such a definition is limited, it suits their objective of investigating the characteristics of communities in which citizen control over legislative bodies, according to the theory of "direct" representation, is likely to emerge.[16]

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[5] Wahlke, "Public Policy and Representative Government. . .," p. 21.

[6] This research is summarized in Wahlke, "Public Policy and Representative Government. . ."

[7] Expressive satisfactions are certainly of some importance also: for example, see Thomas J. Anton, "Roles and Symbols in the Determination of State Expenditures," *Midwest Journal of Political Science*." (1967).

[8] Wahlke, "Public Policy and Representative Government . . . ," p. 33.

[9] Warren Miller and Donald Stokes' model of the relationships between constituency policy preferences and legislators' roll-call behavior permits a statistical estimate of such congruence; see Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," this REVIEW, 57 (1963); and Warren E. Miller, "Majority Rule and the Representative System of Government," in Erik Allardt and Yrjo Littunen (eds.), *Cleavages Ideologies and Party Systems*, Vol. 10 of *Transactions of the Westermarck Society* (Finland, 1964). However, data from the same study suggest that most citizens would hardly be aware of any congruence that might obtain statistically; see Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly*, 26 (1962).

[10] This does not imply that the behavior of the representative is of any diminished importance. The *relationship between* representative and represented is still the phenomenon of interest; and the behavior of the representative is, of course, an integral component of that relationship. Focusing on the represented simply means that the representative-represented relationship is investigated in terms of the represented's perceptions.

[11] Roger H. Davidson, "Congress and the Executive: The race for Representation," in Alfred de Grazia (ed.), *Congress: the First Branch of Government* (New York: Doubleday & Company, Inc., Anchor Books Edition, 1967), p. 373.

[12] Kenneth Prewitt and Heinz Eulau, "Political Matrix and Political Representation: Prolegomenon to a New Departure from an Old Problem," this REVIEW, 63 (1969), p. 441.

The point is that no analytic formulation of representation describes *the* representational relationship. An essentialist definition of representation is, of course, a chimera.[17] Linkages between citizens and political authorities are inextricably bound up with ongoing political systems. Demand-response conceptions of representation may be appropriate to theoretical concern with the problem of citizen control over political authorities, and viewing representation as a relationship between collectivities may be a useful way to approach this problem. However, Wahlke's extensively documented discussion suggests that, if theoretical concern is focused on problems of system persistence and maintenance, demand-response conceptions leave much to be desired, and the most useful way of conceptualizing representational linkages would be as a source of support.[18]

Favorable attitudes and behaviors constitute support for political objects. Easton distinguishes two classes of attitudinal support: specific support involves favorable attitudes in response to outputs which satisfy members' policy demands; diffuse support involves attitudes of general "good will," independent of immediate outputs.[19]

The major objects of the political system toward which support is directed—the domain of support—are identified by Easton as: (1) the political community or that group of persons bound together by the fact that they participate in a common political enterprise; (2) the regime, or the constitutional order, broadly interpreted to include the values, norms, and structures of authority; and (3) the authorities, or those actors occupying roles in the authority structure—the rulers.[20]

Easton argues that the major source of diffuse support for regime and authorities is the belief in legitimacy: a members' sense of the appropriateness of these objects. "It reflects the fact that in some vague or explicit way he sees these objects as conforming to his own moral principles, his own sense of what is right and proper in the political sphere."[21]

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[13] See Heinz Eulau, John C. Wahlke, William Buchanan, and Leroy C. Ferguson, "The Role of the Representative: Some Empirical Observations on the Theory of Edmund Burke," this REVIEW, 53 (1959); and, of course, John C. Wahlke, Heinz Eulau, William Buchanan, and Leroy C. Ferguson, *The Legislative System* (New York: John Wiley & Sons, Inc., 1962).

[14] The quote is from Prewitt and Eulau, "Political Matrix . . . ," p. 430.

[15] Prewitt and Eulau, "Political Matrix . . . ," p. 430.

[16] A comprehensive history of the development of direct representation theory, with particular emphasis on the American setting, is in Alfred de Grazia, *Public and Republic* (New York: Alfred A. Knopf, 1961).

[17] Representation does have an identifiable meaning, which is, in Hanna Pitkin's words, "that in representation something not literally present is considered as present in a nonliteral sense." Hanna Fenichel Pitkin, *The Concept of Representation* (Berkeley; University of California Press, 1967), p. 9. However, the substance of this meaning can be defined in various different ways. For an explication of the various ways in which the concept of representation has been applied to different contexts, see Pitkin, *The Concept of Representation*, passim; also see the Introduction to Pitkin (ed.), *Representation* (New York: Atherton Press, 1969).

[18] Of course, as Wahlke notes, such an approach has been implied in a variety of ways by various analysts. See the discussion in Wahlke, "Public Policy and Representative Government . . . " pp. 29–33.

[19] David Easton, *A Systems Analysis of Political Life* (New York: John Wiley & Sons, Inc., 1965), Ch. 17.

[20] Easton, *A Systems Analysis* . . . , chs., 11–13.

The object toward which specific support is directed is, of course, the political authorities. Easton states that specific support involves "a *quid pro quo* for the fulfillment of demands."[22] However, in considering specific support as an input from the membership in general, and not just from attentive publics, Easton's tendency to equate specific support with demand satisfaction is too restrictive. Surely his broader definition of the concept is the more useful formulation: "Whenever the input of support can be closely associated with the satisfactions obtained from specific classes of output, I shall designate it as specific support."[23] The most useful conception of specific support is not that its distinctive characteristic is demand satisfaction, but simply that it involves members' evaluations of the *performance* of political authorities. Some of the least attentive members may feel that the government (perceived as a monolithic whole) is handling domestic or foreign problems well, and thus extend a form of specific support, without actually entertaining any concrete policy demands, and without being able to identify any particular authorities or groups of authorities as referents for their evaluations. However, an important source of specific support will be the extent to which members feel represented by the performance of particular authorities or groups of authorities occupying roles in the political structures. These representational linkages may result from *performance satisfactions related to the policy-making behavior* of the authorities, or they may result from *satisfactions related to the output of expressive symbols* by authorities which are reassuring to the members in a political performance context. Thus, members' representational linkages will involve beliefs that the performance of authorities or groups of authorities "stands for" or is consistent with their interests, whether instrumental or expressive.

II. MEASUREMENT OF SENSE OF REPRESENTATION

Questions designed to measure sense of representation by Congressman, Congress, President, and Supreme Court, as well as the dimensions of ideological, structural, and personal legitimacy, and intentions to extend behavioral support to the regime, were included on a questionnaire administered during April 1968 to a sample of students enrolled in introductory political science courses at the University of Iowa. Copies of the questionnaire were also mailed to a group of students who had been arrested six months earlier for participating in a protest demonstration against the presence of United States Marine Corps recruiters on the University of Iowa campus. Of 278 students enrolled in introductory political science courses, 275 returned usable questionnaires; of 86 demonstrators to whom questionnaires were mailed, 21 returns were obtained.

For the purposes of this investigation, students enrolled in introductory political science courses and students arrested for participating in the demonstration have been combined into one sample ($N=296$). The resulting sample may be regarded as a crude "scope" sample.[24]

The concern of much social science research, this project included, is not to describe population parameters, but to test general laws, or what Willer terms "conditional universals." Random sampling from finite populations is the appropriate method for collecting data if the purpose is to describe population parameters. For example, data collected by the Gallup and Harris organizations are intended to describe attitudes about political issues, political candidates, means of political change, socioeconomic deprivation, etc., among all Americans at a certain time, or among black Americans at a certain time. But if the purpose of the research project is to test conditional universals, then random samples from finite populations are not necessarily any more useful than nonrandom samples.

The appropriate type of sample for testing conditional universals is the scope sample. As defined by Willer, the scope sample is "a number of natural cases fitting the conditions appropriate to the theory model, which are ranged along the major dimensions of the formal system . . . [for example] If a theory model for the conditions of oligarchy in associations relevant to Michels' 'iron law of oligarchy' were constructed, it should be applied to organizations over a large variation in size, from extremely oligarchical to extremely democratic, and to organizations of varying form and structure, and with members of varying levels of intellectual sophistication . . ." Validating conditional universals requires that the set of generalizations (or "theory model") be tested under as wide a range of circumstances ("ranged along the major dimensions of the formal system") as possible. In effect, the scope sample is a social science surrogate for the experimental method of the natural sciences. In the natural sciences, cases can be arranged by the experimenter himself so as to maximize the variety of the cross-section; in the social sciences, the variety of the cross-section can be maximized only by means of approximately "arranging" cases by scope sampling.

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[21] Easton, *A Systems Analysis* . . . , p. 278.

[22] Easton, *A Systems Analysis* . . . , p. 268.

[23] Easton, *A Systems Analysis* . . . , p. 268.

[24] The notion of a scope sample is discussed in David Willer, *Scientific Sociology* (Englewood Cliffs: Prentice-Hall, Inc., 1967), Ch. 6. The discussion presented in the following paragraphs relies on his explication.

The notion of scope sampling includes samples randomly selected from populations. In some instances, randomly selected samples may provide cross-sections adequate for testing the set of generalizations of interest. However, as Willer points out, "it is essential to remember that the primary aim is to establish the scope of validity (if any) relative to the assumed range of the phenomena. The distribution of real cases [in a random sample] may have nothing to do with the scope." In other words, for the purpose of testing conditional universals, random sampling is valuable only to the extent that it is a better method than nonrandom sampling for maximizing the variation or scope of the sample relative to the range of cases across which it is desired to test the generalizations.[25]

This research project was designed to test the construct validity of a measure of sense of representation.[26] and to explore certain hypotheses about relationships between representation and legitimacy, and the impact of such attitudes on intention to extend behavioral support to the regime.[27] In order to explore and/or test generalizations about support across the entire range of such phenomena, an ideal scope sample in terms of the American setting would include suitable proportions (for multivariate analysis) of, for example, ghetto blacks, college students, middle-class whites, farmers, members of "radical right" organizations, as well as, in all such groups, sizeable proportions of both the politically attentive and inattentive and individuals in the 18–35 years of age category and 35 years of age and older. The present sample is limited to college students. However, within this category, inclusion as cases in the sample of both students enrolled in introductory political science courses and students arrested for participating in a demonstration does, to some extent, increase the range of cases of support-nonsupport available for study, thereby contributing to the utility of the sample with regard to validating the representation measure and investigating relationships between support attitudes and behavioral intention.

The questionnaire included items pertinent to two levels of political objects serving as loci of representational relationships between citizens and political authorities. Congressman and President are objects at the level of representation by *individual* authorities. Representation has most often been considered in terms of such linkages between individuals. However, *groups* of political authorities, such as the 90th Congress or the Warren Court, may also serve as important loci of representational linkages between citizens and government. At this level, representational linkages involve members' perceptions of the collective performance of authorities occupying roles in political structures.

The sense-of-representation-by-Congressman construct was developed from a battery of items as follows. Various research findings have indicated that the visibility of individual legislators is rather low among the general membership in political systems. Therefore, the initial question in the battery was intended as a screening device to distinguish those aware of their Congressman from those not aware of him. Respondents were instructed: "If you do not remember the name of your Congressman, go on to the next set of items. If you do remember the name of your Congressman write it in the following space." The next question requested respondents to make a very general summary evaluative judgment about the performance of their Congressman. This question was designed to measure direction of affect: "Do you feel that your Congressman is basically a pretty good Congressman or do you feel that yo r Congressman is basically not a very good Congressman?" A "Don't Know" option was also included. The cognitive dimension of the respondent's attitude was measured by an open-ended question: "If you answered 'Basically Pretty Good' or 'Basically Not Very Good' above, I would like to know the most important reasons for the way you feel." Finally, intensity of affect was ascertained: "How strongly do you feel this way?—Extremely Strongly: Quite Strongly; Pretty Strongly; Moderately Strongly; Not Too Strongly; Not At All Strongly." Pretesting of this intensity of affect measure disclosed that three-interval options produced poor differentiation, with respondents clustering at the extreme intervals. The six-interval option which was adopted seemed to differentiate more adequately between levels of intensity; for purposes of analysis, "Extremely Strongly" was treated as + or − 3, "Quite Strongly" and "Pretty Strongly" as + or − 2, and "Moderately Strongly," "Not Too Strongly," and "Not At All Strongly" as + or − 1.[28]

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[25] The quotes are from Willer, *Scientific Sociology*, p. 114 and p. 113, respectively.

[26] A preliminary report on these data, written shortly after the fieldwork was completed, appeared under the title "Political Stability and the Habit of Legitimacy," Report #23 from the Laboratory for Political Research, University of Iowa, 1968. The present analysis superscedes that report, which suffered from various conceptual and methodological inadequancies and general lack of refinement.

[27] Legitimacy beliefs are the focus of analysis in Muller, "Correlates and Consequences . . ."

Sense of representation by Congress, by President, and by Supreme Court was measured on the basis of batteries of items equivalent to the above. The initial screening item included in the Congressman series was dropped from these question-sets.

The direction and intensity of affect items are straightforward measures of these attitudinal dimensions. The measure of the cognitive dimension of attitude is intended to differentiate between intervals along a continuum ranging from representation through arepresentation to nonrepresentation.[29]

The cognitive dimension of these attitudes refers to the range of outputs from political authorities which are salient for individuals. For example, with regard to the object of Congressman, there are some citizens who, although they hardly may be apparent in mass sample surveys, do approximate the democratic ideal of the rational citizen—informed, involved, and so on. They would possess detailed information about such matters as their Congressman's positions on the issues and his votes on major legislation. If asked why they favored or did not favor him, their responses would indicate exposure to outputs which could be classified as referential stimuli in the sense that they could be tested concretely against reality, and in the sense that the citizen could relate them to his own policy demands. Another kind of outputs could be classified as condensation stimuli.[30] Individuals responding to condensation stimuli might explain that they favored or did not favor their Congressman because "X is a businessman," or because "X is opposed to immorality in government," or perhaps simply because "X is a Democrat." Such condensation stimuli would be sufficiently imprecise to permit them to take on many different meanings for different people. Too vague to be of instrumental relevance, they would, however, be of significant expressive value. Condensation stimuli would be highly generalized, expressive outputs, not readily susceptible to reality testing, difficult to relate to policy preferences. Finally, a further group of individuals might explain their preference as a result of the influence of their spouse or friends or relatives, or because they liked their Congressman's physical appearance, or simply because they had just always liked or disliked him. Such individuals would not appear to be cognizant of either referential or condensation stimuli. In these cases the range of stimuli affecting attitudes would seem to be relatively devoid of instrumental or expressive content and could therefore be classified as extraneous.

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[28] On intensity of affect toward Congressman, Congress, and the Supreme Court, approximately one-fifth of the respondents scored high or low, three-fifths in the middle (+ or − 2); intensity of affect toward President Johnson was distributed two-sixths high, one-sixth low, three-sixths in the middle.

[29] The intervals along this continuum have been labelled, for consistency and as a mnemonic device: Represented; +Unrepresented; Arepresented; −Unrepresented; Nonrepresented. These are not particularly felicitous terms; they are intended simply as shorthand references to operationally defined characteristics.

[30] "Referential symbols are economical ways of referring to the objective elements in objects or situations; the elements identified in the same way by different people. Such symbols are useful because they help in logical thinking about the situation and in manipulating it . . . Condensation symbols evoke the emotions associated with the situation. They condense into one symbolic event, sign, or act patriotic pride, anxieties, remembrances of past glories or humiliations, promises of future greatness, some one of these or all of them." Edelman, *The Symbolic Uses of Politics*, p. 6. A legislator's vote on a civil rights bill is a referential symbol. Party affiliation for, most citizens, is one of the most salient condensation symbols in political life. It is an economical way, not of referring to the objective elements in objects or situations, but of organizing a whole congeries of basically abstruse and uninteresting information into a coherent entity which has some meaning for the individual whose major concerns in life are not political. The difference between referential and condensation symbols is on the order of more or less; yet it would appear to be of sufficient clarity to permit relatively uniform differentiation between responses indicating perception of outputs characterized mainly by a high degree of referential content, by a high degree of condensation content, or by a high degree of extraneous content.

The distinctions between referential, condensation, and extraneous stimuli are essentially distinctions between the amount of information packed into stimuli and the uses to which this information can be put. Referential stimuli carry a great deal of information which is instrumentally meaningful; condensation stimuli carry much less information, and what there is would be primarily of expressive meaning; extraneous stimuli, in this context, carry little or no instrumentally or expressively meaningful political information.[31]

Thus the question-sets intended to tap the students' affective responses to the performance of political authorities measure the direction and intensity of affect vis-a-vis the performance of the political authorities object (strongly positive; moderately; slightly; neutral; slightly; moderately; strongly negative), and the range of performance outputs associated with the political authorities object (instrumental; expressive; extraneous). These dimensions of attitude were combined to form the sense of representation constructs according to the following rationale.

Sense of representation is assumed to be a continuous variable. At the high positive end of the continuum are attitudes characterized by strongly or moderately positive affect and perception of instrumental or expressive outputs. Such attitudes are considered to be indicative of the presence of positive representational linkages between citizens and political authorities. The citizen entertains either policy demands or emotional desires for symbolic reassurance that political threats are under control; he perceives outputs from the authorities which are relevant to his instrumental

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[31] For example, the following categories were employed for coding the range of perceived stimuli with respect to Congressman:

Perception of Instrumental Outputs. (In each category, some policy content is implied or specified.)

I favor (do not favor) his position or voting behavior on political issues.

He has not carried out his campaign promises.

He has worked for needed projects in the state or district.

He has had many legislative acts passed which have benefitted his district.

His voting behavior has emphasized concern with the problems both of his district and of the nation.

His voting behavior has emphasized national interests above local interests.

His voting behavior has represented (not represented) the majority feelings of his district.

He has voted with his party in Congress.

His voting behavior is too liberal (conservative).

He has served the nation as a watchdog over the economy, defense establishment, etc.

His interests are strongly urban (rural) and he has little understanding of rural (urban) problems.

Perception of Expressive Outputs. (In each category, policy content is unspecified).

He is a member of the same party as I am; he is a member of the other party.

He is a liberal (conservative)—socialist (fascist).

He communicates (does not communicate) often with the people in his district.

He has done a personal favor for me.

Unlike his predecessor, he has been able to get the cooperation of many of the people in his district.

He performs his job well: e.g., he has introduced important legislation; he speaks out well on important problems.

He does not perform his job well: e.g., he is too concerned with petty problems.

He is intelligent and capable: e.g., he went to Harvard; he is a lawyer.

His background suits him for our district: e.g., he is a member of the farm bureau.

He is concerned with great national problems.

He does not follow the party line, but stands up for his own opinion even if it is unpopular.

He is a pawn of those who contributed to his party fund—not independent enough of special interests.

He is carrying out (not carrying out) my beliefs.

He has been an active member of his party in national politics.

He supports (opposes) the present administration.

Perception of Extraneous Outputs.

He has a pleasant (unpleasant) personality—he is a friendly (unfriendly) man.

I like (dislike) him because of the influence of family and friends.

He keeps getting reelected so he must be doing a good job—he does not do anything.

I have not heard many favorable (unfavorable) remarks about him.

He is an admired (not an admired) man.

TABLE 1. DISTRIBUTIONS OF SENSE OF REPRESENTATION

Representation Congressman			Representation Congress			Representation President			Representation Supreme Court		
Group	N	Percent	Group	N	Percent	Group	N	Percent	Group	N	Percent
(R)	88	30	(R)	45	15	(R)	83	28	(R)	93	32
(+UR)	26	09	(+UR)	56	19	(+UR)	64	22	(+UR)	68	23
(AR)	147	50	(AR)	98	33	(AR)	35	12	(AR)	105	36
(−UR)	05	02	(−UR)	20	07	(−UR)	07	02	(−UR)	05	02
(NR)	27	09	(NR)	74	25	(NR)	104	36	(NR)	22	08
Totals	293	100%	Totals	293	99%[a]	Totals	293	100%	Totals	293	101%[a]

[a] Rounding error.

or expressive political interests; and he is strongly to moderately concerned about the performance of the authorities.

At the low positive end of the continuum are attitudes characterized by strongly or moderately positive affect and perception of extraneous outputs; as well as attitudes characterized by slight positive affect and perception of instrumental, expressive, or extraneous outputs. Such attitudes are considered to be indicative of habitual or apathetic support for political authorities: support which is either not of the intensity or not of the rudimentary cognitive sophistication to suggest the presence of politically meaningful representational linkages between citizen and authorities. Low affect intensity, regardless of the type of outputs perceived, is taken to mean that the presence or absence of representational linkages with the authorities is not an important concern of the citizen; perception of extraneous outputs, regardless of the level of affect intensity, is taken to mean that the citizen is unaware of how the authorities might "stand" for him or "represent" his interests (if he has any).

At the midpoint of the continuum are attitudes characterized by, in most instances, the fact that particular political authorities or groups of authorities are simply not visible to the citizen.[32] Attitudes located at the low and high negative intervals are exactly the obverse of attitudes at the low and high positive intervals. To call attention to the distinguishing characteristics of these intervals in a more meaningful way than simply calling them high positive, low positive, etc., they have been labelled as follows: Represented, +Unrepresented, Arepresented, −Unrepresented, Nonrepresented.

Table 1 presents the distributions among these students on the four sense-of-representation scales.[33] The incidence of positive representational linkages with Congressman and Supreme Court is substantially greater than the incidence of negative representational linkages. Negative representational linkages with Congress and President outweigh positive representational linkages, although these students manifest far more positive than negative habitual/apathetic support (the +UR and −UR levels) toward these objects.

Considering representation in legislative bodies, these students show a greater tendency to be unaware of, or not to have an opinion about, their Congressman than is the case with respect to Congress. Most of their neutrality toward their Congressman is due to his lack of visibility, paralleling this phenomenon among the general population.[34] However, among those who do have an opinion, three-fifths feel that his performance represents their interests, and only one-fifth feel that his performance does not represent their interests. By contrast, only about one-fifth of those who have an opinion about the 90th Congress feel that its performance represents their interests, and about two-fifths feel that its performance does not represent their interests. These distributions can be interpreted as underscoring the importance of individual legislator-constituent linkages. To be sure, the majority of these positive representational linkages with Congressman involve perceptions of expressive (77%) rather than instrumental (23%) outputs—these students' images of their Congressman, like those of the general public, are remarkably free of policy content.[35] However, although a Congressman may not be visible to many of his constituents, these data are consistent with the Stokes and Miller finding that "in the main, recognition carries a positive valence; to be perceived at all is to be perceived favorably;"[36] and they suggest that the probability may be high that, if representation is conceived as involving more than just demand-response congruence, a Congressman will be the source of positive representational linkages.

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[32] Unfortunately, screening questions comparable to the Congressman item (e.g., "Have you been following the activities of Congress lately?") were not included in the other question-sets. Thus, the assumption must be made that respondents who selected the "Don't Know" option when requested to make a summary evaluation, did so because they were unaware of the performance of that object. In general, examination of the protocols indicates that this assumption is tenable; however, particularly in the case of President Johnson (judging from comments written in the margins), some respondents who selected the "Don't Know" option did so because their evaluation was neutral, not because they were unaware of the performance of the object.

[33] Three cases could not be coded because respondents reported "favorable" evaluations with the qualification "in terms of the interests of the ruling class," or in terms of the interests of Capitalism [Imperialism]."

[34] See Stokes and Miller, "Party Government . . . ," p. 540; the data reported at p. 340 of

III. CONSTRUCT VALIDITY OF SENSE OF REPRESENTATION

The first priority of this investigation is to test the validity of the representation construct. Construct validation involves deriving from the theory which has generated the construct other constructs which may be expected to be related to the construct of interest, and then formulating and testing the relevant hypotheses.[37] Theoretically, the sense-of-representation construct incorporates distinctions between knowledge about the performance of political authorities and distinctions between intensity of affective response to political authorities. Thus, if the intervals of the sense-of-representation construct actually do entail such distinctions, sense of representation should be nonlinearly related to measures of political knowledge and measures of level of interest in politics, and the form of the relationship should best be described by a quadratic function rule; that is, the relationship should tend to fit the shape of a parabola. The Represented and Nonrepresented should be higher on political knowledge and interest in politics than the +Unrepresented, −Unrepresented, and Arepresented; in turn, the +Unrepresented and −Unrepresented should be higher on political knowledge and interest in politics than the Arepresented.

To test this hypothesis, first the eta coefficient—or correlation ratio—is computed. This coefficient measures the strength of association assuming any form of relationship.[38] Next, the amount of variation that can be explained by linear regression is computed. The difference between the amount of variation explained assuming any form of relationship, and the amount of variation explained assuming a linear relationship, reflects the tendency toward nonlinearity in the data; analysis of variance is employed to test for the presence of nonlinearity.[39] Assuming that nonlinearity is found to be present in each relationship, the method of planned comparisons among means, using orthogonal polynomial weights, can be used to test whether the relationship best fits the hypothesized function rule.[40]

Malcolm E. Jewell and Samuel C. Patterson, *The Legislative Process in the United States;* Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally & Company, 1964), p. 493.

[35] See Stokes and Miller, "Party Government . . ."

[36] Stokes and Miller, "Party Government . . ."

[37] See L. Cronbach, *Essentials of Psychological Testing* (New York: Harper & Row, 2nd ed., 1960).

[38] See J. P. Guilford, *Fundamental Statistics in Psychology and Education* (New York: McGraw-Hill Book Company, 4th edition, 1965), pp. 308–317.

[39] The formula is: $(\eta^2 - r^2)(N - k)/(1 - \eta^2)(k-2)$

[40] A good introductory discussion of this procedure appears in George A. Ferguson, *Statistical Analysis in Psychology and Education* (New York: McGraw-Hill Book Company, 2nd edition, 1966), Ch. 21. For a more comprehensive treatment see William L. Hays, *Statistics* (New York: Holt, Rinehart and Winston, 1963), esp. pp. 539–562 The computing formula used here is that suggested by Hays.

The use of orthogonal polynomial weights, and of course the use of linear regression, is based on the assumption of equal intervals. One good reason for assuming interval measurement, even if such is only approximately the case, is that considerably more information can be gained from the data. Also, even if the data is "truly" ordinal, order-based measures of association leave much to be desired. Gamma always overestimates the degree of association, and is a measure of *one-way* association rather than *co*-relation. Tau-B and Tau-C have no simple interpretation; also, like r, they range between + or − 1 only if the

A composite scale, which is a linear combination of political knowledge and level of interest in politics, was constructed according to the following procedure. Three measures of political knowledge and one measure of interest in politics were subjected to a principal components analysis.[41] With the minimum eigenvalue set at 1.0, the four variables loaded at greater than .500 on one principal factor, and this factor accounted for more than 40 percent of the item variance. Factor scores were then computed according to the complete estimation method.[42] The resulting scale was labelled Attention to Politics.

The relationship between Attention to Politics and the construct intended to measure sense of representation by Congressman is shown in Figure 1. The overall association between Representation Congressman and Attention to Politics is .324 and the explained variation is 10.5 percent. Linear regression accounts for none of this variation. The mean Attention to Politics scores at each Representation Congressman interval suggest that, as hypothesized, the relationship between Attention to Politics and Representation Congressman best fits a quadratic function rule. Those who feel that their Congressman's performance does represent their interests ($\bar{X}=28.48$) or does not represent their interests ($\bar{X}=22.96$), manifest the highest average Attention to Politics scores; a Congressman's habitual/apathetic supporters ($\bar{X}=34.62$) and opponents ($\bar{X}=35.80$) manifest the next lowest average Attention to Politics scores; least of all attentive to politics is the individual whose Congressman is not a salient political object ($\bar{X}=41.08$). Comparison of these means by the method of orthogonal polynomials shows that the moderate nonlinear correlation between these two variables is due, for the most part, to a quadratic trend.

A linear trend is described, of course, by a straight line—or a "curve" with no bends. A quadratic trend is described by a curve with one bend; a cubic trend by a curve with two bends, a quartic trend by a curve with three bends; etc. Since Representation Congressman is a five-point scale, a quintic trend would fit the means exactly—and provide no information at all about the relationship, simply because *any* set of means for a five-point scale can be fitted exactly by a quintic function rule. The best description of any relationship between two variables is provided by the lowest order function rule—or trend—which best fits the distribution of means or points. The analysis of variance table for the relationship between Attention to Politics and Representation Congressman shows that the F test for the quadratic trend is statistically signifi-

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bivariate distribution is perfectly congruent. Interval statistics have been used in this analysis because it was felt that they would best describe the data.

A word is also in order with respect to the use of tests of statistical significance in this analysis. Strictly speaking, they do not apply to this sample. *But,* they do serve as a useful—if arbitrary—guide to the acceptance of *some* relationship, however slight; or to the rejection of a relationship, as being too slight to matter, in terms of what might be expected from further replication.

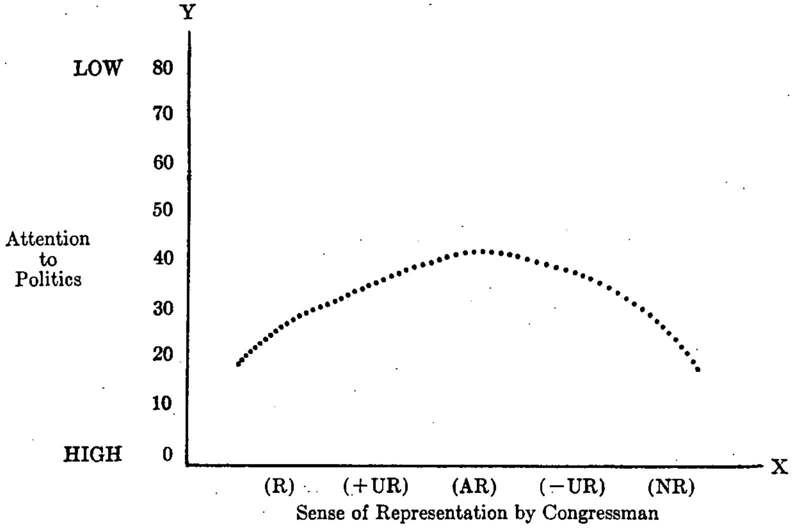
[41] A 4-point General Political Knowledge scale was constructed from the sum of correct responses to the following three items:

(1) Do you happen to remember the names of any of the Chairmen of the Congressional Committees?
(2) Which political party elected the most members to the House of Representatives in 1966?
(3) If you remember how many justices there are on the Supreme Court, write the number in the following space.

In order to measure the respondent's awareness of Congressional policy-making activities, a list of eleven policy items was posed and respondents were asked to report whether Congress passed or did not pass each item during its 1967 session (e.g., "a bill authorizing the appropriations requested by the President for Rent Supplements," etc.); a similar procedure was followed in order to measure the respondent's awareness of Presidential policy-making, in this case, respondents being asked to report whether President Johnson did or did not propose each item (e.g., "a resolution favoring the principle of greater economic assistance to Latin American," etc.). The items were developed from sections reporting the policy activities of the Presidency and the Congress in the Spring, 1968 *Congressional Quarterly Guide to Current American Government.* There was no overlap between the Congressional and Presidential policy items. The resulting Congressional Policy Knowledge and Presidential Policy Knowledge scales were 4-point scales based on quartiles of correct responses. The measure of interest in politics is a 4-point scale based on respondents' reports of how often they read about public affairs in magazines: "Nearly Every Day" through "Very Rarely."

[42] See Harry H. Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 2nd edition, 1967), pp. 350–360.

FIG. 1. Attention to Politics and Sense of Representation by Congressman



$\bar{X}$ = 28.48 34.62 41.08 35.80 22.96

Source of Variation	Degrees of Freedom	Mean Square	F	
Quadratic	1	5109	13.24	$p < .001$
Other	3	2664		
Within	288	386		
Total	292	$\eta_{xy} = .324$	$\eta_{xy}^2 = .105$ $r^2 = .005$	

cant at the .001 level. The mean square for other trends of 2664 is considerably less than the mean square for the quadratic trend, which is 5109. Therefore, it is apparent that the relationship best fits a quadratic function rule; no higher order function rules will provide a better description.[43]

Figure 2 shows the relationship between Attention to Politics and sense of representation by Congress. The mean Attention to Politics scores at each Representation Congress interval again suggest that the hypothesized quadratic function rule best describes the relationship between the two variables. The eta coefficient of .297 indicates a moderate overall correlation. The F test for nonlinearity is statistically significant at the .001 level. The analysis of variance table shows that the F test for the quadratic trend is also statistically significant at the .001 level. Again, the mean square for other trends of 1608 is considerably less than the quadratic mean square, which is 6094. It is apparent that no function rules of higher order than the quadratic will provide a better description of the relationship.

The quadratic trend, as measured by the correlation ratio, is less strong for the Attention to Politics-Representation Congress relationship than it is for the Attention to Politics-Representation Congressman relationship. Those who feel that the performance of the 90th Congress does represent their interests ($\bar{X}=30.53$) or does not represent their interests ($\bar{X}=26.64$), manifest the highest average Attention to Politics scores; and those for whom the 90th Congress is not a salient political

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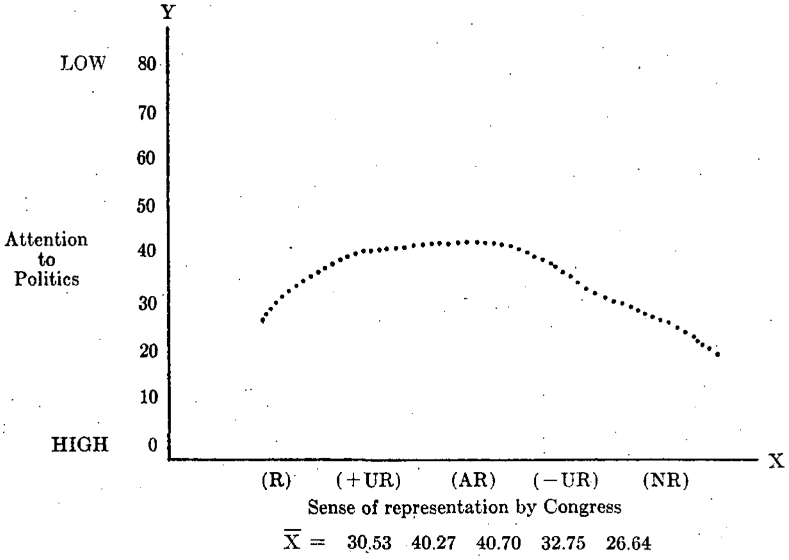
[43] Since unequal numbers of cases are located at each interval on these sense of representation scales, only one trend component can be tested by the procedure of orthogonal polynomials. However, this is no constraint here, because the purpose *is* to test only one trend component (of a higher order than the linear); the possible power of any cubic and quartic trends can be assessed from inspection of the other trends mean square—but there is no need to separately test such trends. Trend analysis with unequal n's is discussed at pp. 351–352 of Ferguson, *Statistical Analysis* . . .

FIG. 2. Attention to Politics and Sense of Representation by Congress



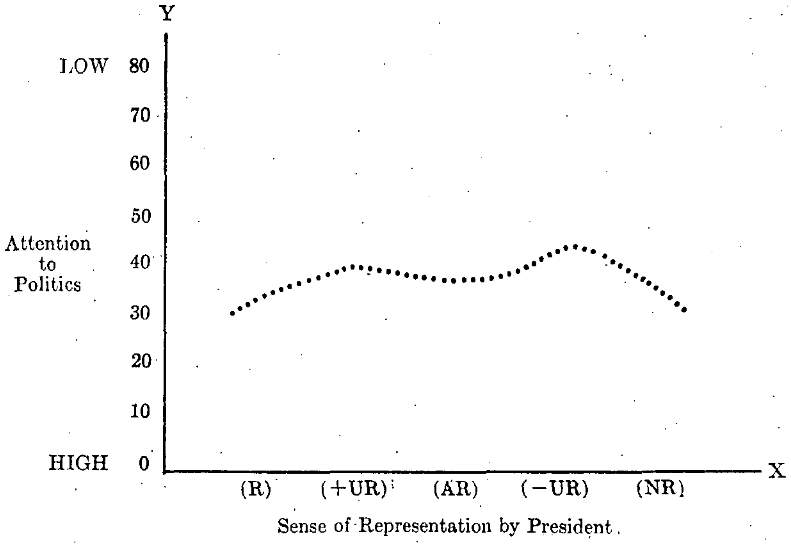
Source of Variation	Degrees of Freedom	Mean Square	F	
Quadratic	1	6084	15.44	$p<.001$
Other	3	1608		
Within	288	394		
Total	292	$\eta_{xy}=.297$	$\eta_{xy}^2=.088$ $r^2=.020$	$F=7.15$ $p<.001$

object ($\bar{X}=40.70$) are the least attentive to politics. However, the mean Attention to Politics score for habitual/apathetic supporters of the 90th Congress ($\bar{X}=40.27$) is practically the same as the mean score for those located at the Arepresented interval, and the mean for habitual/apathetic opponents of the 90th Congress ($\bar{X}=32.75$) is not very much lower than the mean for those located at the Represented interval. Thus, while the relationship between Attention to Politics and Representation Congress does fit the shape of a parabolic curve, as hypothesized, it is also evident that the intended distinctions between the Represented Congress intervals are somewhat less clear than in the case of the sense of representation by Congressman construct.

Figure 3 shows the relationship between Attention to Politics and Representation President. Of the four measures of sense of representation, the intended distinctions between intervals are least clear with respect to the object of President. The eta coefficient of .151 indicates a slight overall correlation. Linear regression obviously accounts for none of the explained variation. The analysis of variance table shows that the F test for the quadratic trend is statistically significant, but only at the .05 level. The mean square for other trends is even smaller than the mean square within groups—the error variance—thus indicating that other trends are completely irrelevant.

Since the quadratic trend for the Attention to Politics-Representation President relationship is statistically significant at the .05 level, the Representation President measure has been used in the investigation of relationships between representation, legitimacy, and behavioral support, which follows this section. Really, however, with an eta as low as .151, the only justification for using the Representation President measure is that this is an "exploratory" study. It is clear where the problem lies. The mean Attention to Politics score for the Arepresented ($\bar{X}=37.69$) is *higher* than the mean scores for President Johnson's habitual/apathetic supporters ($\bar{X}=39.11$) and opponents ($\bar{X}=43.29$), whereas, if the distinctions between these intervals were as intended, the opposite would be the

FIG. 3. Attention to Politics and Sense of Representation by President



$\bar{X}$ = 32.15 39.11 37.69 43.29 33.18

Source of Variation	Degrees of Freedom	Mean Square	F	
Quadratic	1	2177	5.16	$p < .05$
Other	3	219		
Within	288	422		
Total	292	$\eta_{xy} = .151$	$\eta_{xy}^2 = .033$ $r^2 = .000$	

case. As expected, those who feel that President Johnson's performance does represent their interest ($\bar{X} = 32.15$) or does not represent their interests ($\bar{X} = 33.18$), manifest the highest average Attention to Politics scores. But it seems likely that for many individuals located at the Arepresented interval, President Johnson is a salient political object—but at the same time an object of neutral evaluation.

Turning to Figure 4, the relationship between Representation Supreme Court and Attention to Politics is strongest of all. The eta coefficient of .398 indicates the presence of a relatively strong relationship. The F test for nonlinearity is statistically significant at the .001 level. The other trends mean square of 4706 is less than the quadratic trend mean square of 5534. Thus, as hypothesized, the relationship between Attention to Politics and Representation Supreme Court best fits the shape of a parabolic curve.

The data presented in Figures 1–4 confirm the construct validity of Representation Congressman, Representation Congress, and Representation Supreme Court. The construct validity of Representation President is questionable.

IV. REPRESENTATION, LEGITIMACY, AND BEHAVIORAL SUPPORT

In order to maintain itself in a relatively stable state the political system must be able to cope with those disturbances which produce stress. Support, Easton argues, "becomes the major summary variable linking a system to its environment."[44] One of the major conditions of political stability-instability, then, is the ability of the political system to generate and maintain minimal levels of affective and behavioral support on the part of its members for the central political objects: authorities, regime, and community.

Support most immediately relevant to the stability-instability of political systems is that which Easton terms overt support—supportive behavior. Withdrawals of behavioral support directly stress political systems by inhibiting the conversion of demands into outputs by the

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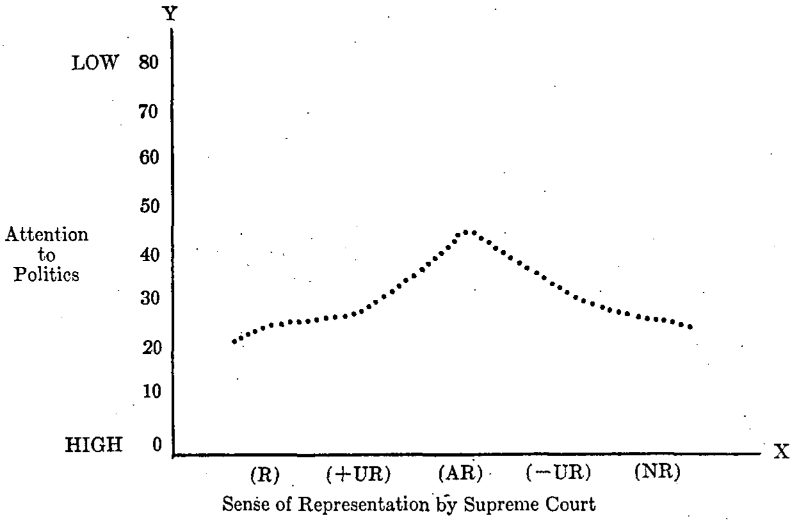
[44] Easton, *A Systems Analysis* . . . , p. 156.

FIG. 4. Attention to Politics and Sense of Representation by Supreme Court



$\overline{X}$ = 24.71 28.47 43.54 30.20 27.59

Source of Variation	Degrees of Freedom	Mean Square	F	
Quadratic	1	5534	15.20	$p < .001$
Other	3	4706		
Within	288	364		
Total	292	$\eta_{xy} = .398$	$\eta_{xy}^2 = .158$ $r^2 = .036$	$F = 13.35$ $p < .001$

authorities, threatening the persistence of regimes, and eventually producing complete collapse of the system. Associated with the withdrawal of behavioral support are the turmoil and revolutionary dimensions of civil strife.[45] Thus the survival of authorities, regime, and the political community itself depends on the effective development and maintenance of behavioral support. An important question, then is to ascertain the sources of behavioral support. Sense of representation and legitimacy are obviously two antecedents worth investigating.

The four sense-of-representation variables have been discussed. Following Easton's conceptualization of the dimensions of legitimacy, measures of ideological, structural, and personal legitimacy have been constructed.[46] Regime values are the objects of ideological legitimacy. Legitimating ideologies are sets of values which validate the power of the authorities and regime. In Dahl's discussion of the prevailing American ideology, a set of five such values is identified: (1) popular government; (2) the constitutional system; (3) observance of legal processes of change; (4) the virtues of private property; (5) equal opportunity for all to succeed.[47] The Agree Legitimating Ideology variable is a summated scale constructed from items measuring each of these values.[48] Congress and the Supreme Court are the objects of structural legitimacy included in this investigation. Legitimacy Congress and Legitimacy Supreme Court are variables intended to measure the degree

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[45] A useful discussion of these dimensions is in Ted Gurr, "Psychological Factors in Civil Violence," *World Politics*, 20 (1968), 272–277.

[46] These dimensions are discussed in Chs. 18 and 19 of Easton, *A Systems Analysis* . . .

[47] See Robert A. Dahl, "The American Oppositions: Affirmation and Denial," in Robert A. Dahl (ed.), *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966).

[48] The construction of these variables is discussed at length, and the exact wording of the questions reported in Muller, "Correlates and Consequences . . ."

TABLE 2. CORRELATIONS (R) BETWEEN SENSE OF REPRESENTATION, LEGITIMACY, AND BEHAVIORAL SUPPORT

	Sense of Representation			Legitimacy				Support
	Representation Congress	Representation President	Representation Supreme Court	Legitimacy Congress	Legitimacy Supreme Court	Agree Legitimating Ideology	Trust Government	Avoid Protest Action
Representation Congressman	.047	.127	−.032	.179	.105	.204	.174	.218
Representation Congress		.188	−.010	.357	.118	.241	.295	.166
Representation President			.006	.207	.125	.214	.385	.289
Representation Supreme Court				.058	.403	−.008	.053	−.097

to which these institutions are felt to be morally appropriate. The political authorities, of course, are the objects of personal legitimacy. Personal legitimacy sentiments may be directed toward particular individuals; the phenomenon of charismatic leadership is an example. General feelings of trust-distrust in the political authorities considered collectively are an equally if not more important source of personal legitimacy sentiments in political systems. The measure of personal legitimacy, Trust Government, is based on the SRC political trust items. Finally, Avoid Protest Action, the measure of behavioral support, has been constructed from items measuring behavioral intentions to avoid joining nonviolent protest demonstrations, avoid joining violent protest demonstrations, and avoid violating laws.

Table 2 shows the intercorrelations between the sense of representation measures, and the correlations between sense of representation and legitimacy and behavioral support. There is some tendency for students to associate their sense of representation by their Congressman with their sense of representation by President Johnson, and to associate their sense of representation by the 90th Congress with their sense of representation by President Johnson; but these correlations are of relatively slight magnitude. Representation Congressman and Representation Congress are quite unrelated: the performance of individual Congressmen and the collective performance of the 90th Congress provide independent sources of representational linkages with this legislative institution. Also, the performance of the Warren Court is not connected in any way to the performance of the other three objects.

The clear tendency is for individuals to perceive the performance of any one political authority or group of authorities as distinct from the performance of other authorities. The sense of representation relationships tend not to overlap.

Through processes of political socialization, legitimacy sentiments are learned at an early age. At least in the Amercian political system, the high-school age child has developed quite firmly entrenched ideological, structural, and personal legitimacy sentiments.[49] But, as the child matures, these legitimacy beliefs do not exist in a vacuum. The performance of particular political authorities and groups of authorities may be expected, to some extent, to buttress or contribute to the erosion of legitimacy beliefs.

Of all the authority level objects included in this research design, the performance of the individual Congressman is least consequential in the overall determination of outputs. Certainly, some Congressmen wield substantial political power by virtue of committee chairmanships, etc. But, of course, it is the performance of the President, Congress as a whole, and the Supreme Court as a whole which is most consequential in the determination of outputs. Since legitimacy is a very general or diffuse type of affect, initially learned quite independently of specific performance, it would be expected that members would tend to associate only the performance of the most consequential authority level objects with their legitimacy beliefs. Table 2 shows that, among those students, this is in fact the case.

The correlations between Representation Congressman and legitimacy are slight: only the relationship with Agree Legitimating

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[49] See, for example, David Easton and Robert D. Hess, "The Child's Political World," *Midwest Journal of Political Science*, 6 (1962); David Easton and Jack Dennis, "The Child's Image of Government," 361 *The Annals*, (1965).

Ideology reaches the .200 level. On the other hand, the correlations between Representation Congress and legitimacy are the largest of all. As would be expected, the collective performance of the 90th Congress is most strongly associated with the degree of legitimacy accorded to this institution. Sense of representation by Congress also makes a moderate contribution to the buttressing or erosion of diffuse trust in the political authorities, and to agreement-disagreement with the legitimating ideology. Other research suggests that Americans may regard Congress as the focal point of their government.[50] Prior investigation of these data has indicated that, in terms of psychological constraint between legitimacy beliefs, and in terms of impact on behavioral support, Congress is apparently a more important regime structure than the Supreme Court.[51] The correlations between Representation Congress and legitimacy underscore the centrality of this institution with respect to levels of diffuse affective support for authorities and regime.

The sense of representation which most strongly buttresses or erodes diffuse trust in the authorities is that with the President: the correlation between Representation President and Trust Government is .385. Otherwise, the correlations between Representation President and legitimacy are rather small.

These students associate the collective performance of the Warren Court with the legitimacy which they accord to the Supreme Court as a regime structure. However, absolutely no connection is made between the collective performance of the Warren Court and the other objects of legitimacy sentiments. Also, there is hardly any relation to speak of between the legitimacy accorded to the Court and the other objects of representational linkages. Although support for the Supreme Court is high, the Court is apparently perceived as a rather remote institution, with little consequence for affect directed toward other political objects.

The gross effects of sense of representation on behavioral support are generally moderate to slight; sense of representation by the Supreme Court has no effect on behavioral support. In contrast to the case of legitimacy sentiments, it is Representation President, not Representation Congress, which is associated most closely with behavioral intention to avoid or engage in various types of disruptive protest action. Of the three sense of representation relationships which do affect Avoid Protest Action, Representation Congress shows the least gross impact. The moderate impact of Representation President is not surprising; because the President is such a powerful political object in the American system, his performance likely would have some effect on behavioral support—especially so in the case of an extremely active and visible President like Lyndon Johnson. Yet, given the relevance for affect toward other political objects shown by support for Congress, it might also be expected that the magnitude of the Representation Congress-Avoid Protest Action correlation would approach, if not surpass, the size of the Representation President-Avoid Protest Action Correlation. But, among these students, it is their sense of representation by individual authorities that has the most effect on their intentions to avoid or engage in disruptive protest action. Perhaps, however, the effect of Representation President on Avoid Protest Action might fluctuate to some extent, depending on the personality of the particular occupant of this office.

Of course, none of the separate correlations between sense of representation and behavioral support for the regime are particularly sizeable. Excluding the Supreme Court in both instances, the average correlation between sense of representation and Avoid Protest Action is .224, whereas the average correlation between legitimacy and Avoid Protest Action is .373. Not surprisingly, legitimacy sentiments are a more important source of behavioral support for the regime than sense of representation. However, the relevance of sense of representation is by no means negligible.

A major purpose of this project was to test the construct validity hypothesis. A second hypothesis is: *the effect of legitimacy beliefs on behavioral support will be strong and largely direct, the effect of sense of representation by political authorities will be, for the most part, indirect, via legitimacy, and legitimacy and sense of representation taken together will have relatively substantial impact on intentions to extend or withdraw behavioral support.* In other words, there should be a relatively strong tendency for legitimacy and behavioral support to covary positively, regardless of sense of representation; there should be an even stronger tendency for legitimacy and representation together to covary positively with behavioral support; but there should also be a slight tendency for individuals high on legitimacy to be low on behavioral support if they are also low on sense of representation and, conversely, for individuals low on legitimacy to be high on behavioral support if they are also high on sense of representation.

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[50] See Robert E. Lane, *Political Ideology* (New York: The Free Press, 1962), p. 146; Easton and Dennis, "The Child's Image of Government," pp. 48–49.

[51] See Muller, "Correlates and Consequences . . ."

From Table 3 it can be seen that the multiple correlation between representation, considered separately, and Avoid Protest Action is .370—a moderate total impact, accounting for 13.7 percent of the variance in Avoid Protest Action. It is apparent that the effect of representation on behavioral support is to some extent cumulative. While no single representation linkage correlates with Avoid Protest Action at greater than .289, taking all three into account does produce a measurable increase in the gross effect. However, only about 50 percent of this total impact is attributable to direct effects, as shown by the multiple-partial correlation of .190; the other 50 percent is indirect, depending on the extent to which an individual's sense of representation buttresses or erodes his legitimacy sentiments. Thus the impact of sense of representation on behavioral support is more direct than hypothesized; nevertheless, a considerable portion of the total effect is indirect.

The multiple correlation between legitimacy, considered separately, and Avoid Protest Action is .472—a sizeable total impact, accounting for 22.3 percent of the variance in Avoid Protest Action. And about 75 percent of this total effect is direct, as shown by the multiple-partial correlation of .363. Although it was expected that legitimacy might account for more of the variance than is the case here, the relationship is, nevertheless, fairly strong. Also, as hypothesized, direct effects comprise by far the major portion of the total impact of an individual's beliefs on his intentions to extend or withdraw behavioral support.

TABLE 3. THE IMPACT OF SENSE OF REPRESENTATION AND LEGITIMACY ON BEHAVIORAL SUPPORT

	Behavioral Support Avoid Protest Action	
	multiple R	multiple-partial r
Representation Congressman Representation Congress Representation President	.370	.190
Legitimacy Congress Agree Legitimating Ideology Trust Government	.472	.363
Representation + Legitimacy	.501	

The multiple correlation between representation and legitimacy, taken together, and Avoid Protest Action is .501—accounting for 25.1 percent of the variance in Avoid Protest Action. This can be interpreted as evidence that representation and legitimacy together have a relatively substantial impact on behavioral support—given that some of these measures are rather crude (particularly the dependent variable, which could stand improvement with respect to the number of items used to construct the measure); that the condition of a multivariate normal correlation surface (that is, distributions of congruent shape) is not met; and that the explanation of behavior at the level of individuals is more complex and more subject to random variation, than is the explanation of behavior at the level of *aggregate* units of individuals (that is, explanation of the average behavior of numerous individuals).

Of course, adding the sense of representation variables to the regression equation does not increase predictive power much beyond that obtaining for the impact of legitimacy beliefs alone on Avoid Protest Action. However, with respect to *explaining* rather than predicting behavioral support, sense of representation is clearly a variable cluster of some importance. The performance of political authorities certainly affects behavioral support, but much of this effect depends on the extent to which individuals associate this performance with their legitimacy sentiments; and the collective performance of legislative bodies, as well as the performance of powerful individual authorities such as the American President, is associated, if only moderately, with legitimacy sentiments.

Now, if sense of representation and legitimacy do affect behavioral support according to the pattern hypothesized, a question of some import for system stability-instability centers on the magnitude of the indirect effects of representation. Sense of representation was not found to be correlated closely with legitimacy beliefs among these students at the time of this study; nor was sense of representation closely associated with behavioral support. Behavioral support was found to depend mainly on legitimacy beliefs which were not bound up strongly with the political authorities' performance of the moment—as ought to be the case in a well-functioning democracy, where the performance of the authorities ideally ought to be a focus of positive and negative affect, but affect which is not a foundation for the stability of the regime (affect which is not closely bound up with legitimacy). However, if sense of representation were to become correlated highly with legitimacy beliefs and with behavioral support (if sense of representation were to show strong indirect effects on behavioral support via legitimacy), satisfaction-dissatisfaction with the performance of the authorities would become the foundation for regime stability-instability. And such a foundation could be insecure indeed.

V. CONCLUSION

Representation linkages between citizens and political authorities have been conceived as a type of specific or performance support. An operational construct for empirically measuring citizens' sense of representation by authority level objects was proposed. The validity of four sense of representation scales (Congressman, Congress, President, Supreme Court) was tested. It was hypothesized that if the sense of representation measure is a valid construct, the relationship between each sense of representation scale and an Attention to Politics scale should best be described by a quadratic function rule. In each case, the data confirm the hypothesis, although the Representation President-Attention to Politics relationship shows a rather weak fit.

Relationships between representation, legitimacy and behavioral support were investigated. Sense of representation was found to be slightly to moderately associated with legitimacy beliefs and behavioral intention to extend or withdraw support. With respect to buttressing or eroding legitimacy beliefs, sense of representation by Congress was found to have the most important effects. Sense of representation was also found to have slight to moderate gross effects on behavioral support: however, much of this impact was indirect, as hypothesized, depending on the extent to which sense of representation was associated with legitimacy.

DIMENSIONS OF CANDIDATE EVALUATION*

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The story of a presidential election year is in many ways the story of the actions and interactions of those considered as possible candidates for their nation's highest office. If this is true in the abstract, it certainly was true in the election of 1968. The political headlines of 1968 were captured by those who ran for the nominations of their parties, those who pondered over whether or not to run, those who chose to pull out of the race or were struck down during the campaign, those who raised a third party banner, and those who resisted suggestions to run outside the two-party structure. While 1968 may have been unusual in the extent to which many prospective candidates dominated the political scene, every presidential election is, in its own way, highlighted by those considered for the office of President.

The political scientist has shown scholarly interest in the candidates. His interest, however, has been selective in its focus—mainly concentrating on the two actual party nominees and not the larger set of possible presidential candidates. Research in electoral behavior has detailed the popular image of the nominees in terms of the public's reactions to their record and experience, personal qualities, and party affiliation. Furthermore, attitudes toward the nominees have been shown to constitute a major short-term influence on the vote.[1] Yet attitudes toward other candidates have been surveyed only to ascertain the behavior of those people who favored someone other than the ultimate nominees. The focus in the discipline remains on the nominees—feelings toward them and willingness to accept them.

As research moves from an exclusive concern with explanation of the election outcomes, more scholarly attention should be given to popular attitudes toward the full spectrum of possible candidates. Much remains to be learned about voters' perceptions of the candidates. We still know very little about the psychological dimensions of meaning involved in how an individual perceives, reacts to, and evaluates a set of candidates. We know little about the more general organizing concepts a person uses in developing the specific perceptions and reactions described in contemporary voting and public opinion surveys. In this paper, we shall seek a dimensional interpretation of the individual's perceptions of and preferences for candidates. Extending the set of candidates beyond the bounds of the two nominees allows us to search out broader meanings of candidate evaluation.

While little is known about the factors leading to differing patterns of candidate evaluation, the voting behavior literature suggests a variety of factors as relevant guidelines in any initial inquiries. The most obvious long-term factor is partisan identification. This psychological attachment to party has been treated as a major influence on the vote and research has shown that it can color voters' perceptions of the nominees. The various twists such a factor can take with a larger set of candidates will be a continuing theme of this paper.

Additional factors which may affect candidate evaluation include a person's ideology, the issues of the day, and the personality of the candidates. The crucial task is to distinguish which of these factors are important. A related theoretical question is the number of dimensions used as bases for candidate evaluation.

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* This is a revised version of a paper presented at the Annual Meeting of the American Political Science Association, New York City, September, 1969. We are grateful to Warren E. Miller, George B. Rabinowitz, and Stuart Rabinowitz for their valuable advice and comments. Professor Weisberg acknowledges the fellowship support of the Horace H. Rackham School of Graduate Studies of The University of Michigan during the preparation of this article. Professor Rusk was affiliated with The University of Michigan's Survey Research Center as co-director of its 1968 election study at the time this article was written. This paper is based on the Survey Research Center's 1968 election study which was made possible by a grant from the Ford Foundation.

[1] See, for example, A. Campbell, G. Gurin, and W. E. Miller, *The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954); A. Campbell, P. E. Converse, W. E. Miller, and D. E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960); D. E. Stokes, "Some Dynamic Elements of Contests for the Presidency," this REVIEW, 60 (March 1966), 19–28.

The pioneering work on the modelling of party competition by Anthony Downs was based on a unidimensional ideological continuum.[2] As Stokes has pointed out, Downs's notion of a unidimensional political space is only an assumption, since party competition could instead easily roam over a multidimensional space.[3] The problem of the number of dimensions should be seen as an empirical question. Evidence from surveys indicates that the American public often sees issues in multidimensional terms. Most researchers using dimensional analysis techniques on European multi-party systems have also found the assumption of a single ideological dimension to be inadequate. A prime example is Converse's skillful analysis of the French political scene which found two dimensions being used for the evaluation of that country's many parties.[4] Exploration of the dimensionality of perceptions of candidates in the United States may be one means of testing the dimensionality of the competition space within which national choices of leadership are made.

I. THE THERMOMETER QUESTION

The specific focus of this study will be on individual reactions to a set of twelve candidates for national office. Data will be taken from the interviews of over one thousand respondents in the 1968 election study of the University of Michigan's Survey Research Center. A measure was needed to obtain the feelings of the respondents toward the several candidates. We felt that the respondent should be allowed to use those dimensions which come naturally to him, which are his normal guidelines for thinking about candidates. By obtaining such responses without imposing a frame of reference, it becomes possible in the analysis to deduce the dimensions of importance in the thought of individuals. We have employed a measuring device called the "feeling thermometer" which provides one such neutrally worded means of eliciting responses to a wide variety of candidates.

Basically a "feeling thermometer" is a question asking respondents to indicate on a 0-to-100-degree temperature scale how warm or cold they feel toward a set of objects—in this case, candidates.[5] If a person feels particularly warm or favorable toward a political figure, he would give that candidate a score somewhere between 50 and 100, depending on how warm his feeling was toward that candidate. If he felt cold or unfavorable toward that candidate, 0 to 50 degrees would be the appropriate scoring range. The actual score of 50 degrees was explained to the respondent to be for candidates about whom he felt neither particularly warm nor cold, a neutral point on the scale. To make the thermometer scale more concrete, a card listing nine temperatures throughout the scale range and their corresponding verbal meanings as to intensity of "hot" or "cold" feelings was handed to the respondent. A separate statement in the question wording attempted to elicit "don't know" responses to individual candidates when appropriate.

The twelve people the respondents were asked to rate on the feeling thermometer covered a wide range of possible presidential hopefuls and ideological streams. The names listed included the actual presidential nominees themselves—Nixon, Humphrey, and Wallace—and their vice-presidential running mates. Lyndon Johnson was included, being the incumbent President and considered at one time to be a candidate for reelection. The other five mentioned were the main presidential hopefuls discussed at length in the media who failed to get their parties' nominations—Eugene McCarthy, Ronald Reagan, Nelson Rockefeller, George Romney, and the late Robert Kennedy. This list is somewhat arbitrary, as any list of presidential hopefuls must be. Having the respondents rate twelve candidates is a reasonable task in a survey setting and yet provides enough information for a dimensional analysis of the candidate space. The vice-presidential nominees were included on a basically presidential-oriented list because of the use of the vice-presidential candidates as an issue in the 1968 campaign and the fact that they are increasingly considered to be an integral part of the presidential race.

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[2] A. Downs, *An Economic Theory of Democracy* (New York: Harper and Brothers, 1957).

[3] D. E. Stokes, "Spatial Models of Party Competition," this REVIEW, 57 (June 1963), 368–377.

[4] P. E. Converse, "The Problem of Party Distances in Models of Voting Change," in M. K. Jennings and L. H. Zeigler (eds.), *The Electoral Process* (Englewood Cliffs, N.J.: Prentice-Hall, 1966).

[5] The thermometer question followed the basic format devised by A. R. Clausen for previous Survey Research Center studies, but was revised by the authors to apply to candidates rather than groups and to screen out "don't know" responses. The full wording of the question is given in the appendix. For other analysis involving this question in the 1968 election study, see P. E. Converse, W. E. Miller, J. G. Rusk, and A. C. Wolfe, "Continuity and Change in American Politics," this REVIEW, 63 (December 1969), 1083–1105.

The responses to the twelve candidates can initially be conceived of as indicating each individual's feelings toward each given candidate. Additionally, we shall interpret the relative scores given to the candidates as indicating a person's preference order for these twelve candidates. A respondent is assumed to prefer most the candidate to whom he gives the highest score, and so on. While it must be admitted that there may not be a perfect correspondence between an individual's relative scores and his preference order, the use of the thermometer question constitutes one of the simplest means of obtaining preference orders over a large number of alternatives in a survey of the mass public. More conventional ranking of a dozen candidates would constitute a very difficult task for many respondents, whereas scoring the candidates on the thermometer scale was generally painless.

It should be pointed out that the thermometer question was asked *after* the election. This timing is likely to have affected the ratings of the candidates. A "bandwagon effect" in favor of president-elect Nixon must be considered likely. Humphrey was also probably evaluated much more favorably at this point of time than he would have been during the early part of the campaign, due to a combination of the increased party unity on his behalf and sympathy for his defeat. The tragedy of the assassination of Robert Kennedy could be expected to yield a more favorable rating for him than would have been obtained from a measurement during the period in which he was actively campaigning. Finally the vice-presidential candidates were probably near their peak of saliency at the moment of this study. The timing of the question will influence our measure of the relative appeal of the candidates, but we shall be able to minimize its effect on the dimensional analysis.

The first thing to explore about the data obtained from the thermometer question is their basic statistical parameters. By statistical parameters we mean the level of salience of the candidates and the differences in assessments of them as reflected in their central tendency and dispersion figures. Table 1 summarizes such descriptive features for the candidates included in the 1968 study.

An initial glance at the percentage of "don't know" replies in Table 1 comments to some extent on the salience of each candidate. The names of the candidates in 1968 were generally recognized by the public. The question was deliberately worded to invite "don't know" replies to the candidates when relevant, but very few were given. The greatest proportion of "don't know" responses was only 8 percent for Romney and Muskie. The only other candidates receiving more than 6 percent "don't know's" were the remaining vice-presidential candidates. In particular, Humphrey, Nixon, Wallace, Johnson, and Kennedy were nearly universally recognized.

TABLE 1. SUMMARY OF CANDIDATE THERMOMETERS

Candidate	Don't Know	50°	Standard Deviation	Mean	First Choice Mentions*	Last Choice Mentions*
Kennedy	1%	13%	26	70.1	43%	8%
Nixon	1	16	23	66.5	36	8
Humphrey	1	14	27	61.7	25	13
Muskie	8	31	22	61.4	16	10
Johnson	1	15	26	58.4	17	14
McCarthy	5	32	23	54.8	11	14
Rockefeller	4	30	22	53.8	9	15
Agnew	7	41	21	50.4	4	13
Reagan	5	34	22	49.1	5	17
Romney	8	46	19	49.0	3	15
LeMay	7	29	26	35.2	3	40
Wallace	2	13	31	31.4	11	62
N's	1315–1326	1210–1311	1210–1311	1210–1311	1304	1304

* These columns add up to more than 100 percent because a respondent could give the same highest or lowest score to several candidates.

Although the surface level of salience was high, a substantial number of the thermometer scores did not indicate whether the respondent felt particularly warm or cold toward a given candidate. A score of 50 degrees could indicate either ambivalent feelings or no opinion. The proportions of 50's for the presidential nominees, the incumbent President, and the late Senator Kennedy were all quite small, but the other candidates showed a sizable concentration of such scores. The large number of 50 scores reflects a substantial lack of affective feeling toward some of the candidates and, in turn, qualifies our earlier discussion of their saliency. Some of the candidates are perceived in the most superficial terms while others have a more central place in the respondents' cognitive structures. There appears to be a major threshold for public evaluation of the candidates which can be passed only through an event of great importance such as the actual nomination. This threshold has implications for the subsequent analysis in that the differences in evaluations of the candidates could be related to their differences in saliency.

In general, the candidates were favorably perceived. The mean values in Table 1 show that most of the twelve political figures were given thermometer scores greater than 50, the break-even or neutral point on the scale.[6] Only

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[6] Mean values of all the candidates were drawn

George Wallace and his running mate Curtis LeMay scored significantly below 50. In fact over 60 percent of the sample gave George Wallace the lowest score that they gave to any of the candidates, a strong indication that he never succeeded in making a serious appeal to a majority of the electorate. On the popular side of the ledger, the late Robert Kennedy led the field. The two major presidential candidates also ranked high as did Humphrey's running mate. Lyndon Johnson was also seen in fairly positive terms.

The reasons for the generally positive perceptions are not completely clear and cannot be directly ascertained from the thermometer data. One necessary condition for a very favorable rating is the salience of the candidate. The candidates with the most favorable images were particularly well-known. A closely related argument is that the presidential race draws the better men in politics—men with a good image, record, and qualifications and the media publicity and coverage associated with these attributes. A third explanation might be the prestige and dignity associated with seeking the highest office in the land, though some deviant cases exist to question the generality of such a conclusion. Because the thermometer readings were taken after the election, it is difficult to know what explanation might best fit the major party nominees. They may have been nominated because of their public popularity or they may have achieved public popularity because of their nominations and the ensuing campaign and election.

As much as candidates were perceived favorably, individuals still were able to discriminate among those placed before them. Two types of discrimination resulted—variation of scores within candidates across all individuals and variation of an individual's scores across candidates. The standard deviation figures in Table 1 attest to the first type of discrimination. These figures show the considerable fluctuation in "feeling" recorded for each candidate across the set of respondents; such values range from 19 degrees for Romney to 31 degrees for Wallace. A case-by-case inspection reveals the second type of discrimination; the set of twelve candidates was invariably perceived differently by any given individual. The average extent of the range of scores given by a respondent was 73 degrees out of the possible 100. The standard deviation of the values given by the average respondent was 23 degrees, compared to a theoretical maximum of 50 degrees. These two types of discrimination provide the ingredients necessary to justify any analysis of candidate evaluations.

In general, the statistical parameters of the thermometer question point to the recognition and positive evaluation of the candidates along with considerable discrimination among them. A notion of those liked and disliked was gained—a popularity scale was indeed in evidence and it showed the critical position of the presidential nominees. But the basis of these evaluations is yet to be explored. A search beyond the descriptive parameters of the thermometer scores to their underlying dimensions is needed in order to detect the antecedents of candidate evaluation.

II. THE CANDIDATE SPACE

We shall begin a consideration of the dimensional properties of candidate evaluation with an analysis of the three actual presidential nominees by themselves. It is reasonable to expect that their central position in the campaign would accord them critical positions in the overall candidate space which we seek to describe. Then we shall turn our attention to the full set of candidates as a means of noting the relative positions they are assigned in that space.

Table 2 indicates the relative frequencies for the various preference orders for the three nominees.[7] A basic question is whether a single dimension can account for these preferences. A necessary condition for unidimensionality is that all the preference orders end with only two of the alternatives. If the respondents do employ only a single dimension, then their last place choice must be one of the two extreme alternatives on that dimension.[8] If, on the other hand,

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closer to the break-even point of 50 on the scale because of use by some respondents of the score of 50 degrees to indicate no feeling about the candidates, a meaning which was other than the intended meaning of neutral feelings. The differential in means between Wallace and LeMay would be all but erased if this factor were corrected by removing all 50 responses while the Muskie-Humphrey difference would be dramatically reversed. If the candidates given large numbers of 50's were better known, it is possible that they would have been received in much more favorable or unfavorable terms as the public would have been better able to judge them.

[7] The relationship between these preference orders and the vote may be of some interest. Of those voters giving different scores to the three nominees, about 94 percent voted for the candidate they ranked highest while less than one percent voted for the candidate they ranked lowest. About 97 percent of those giving the two major party nominees their highest two rankings voted for their first choice, compared to only 82 percent of those giving Wallace one of their top two scores.

[8] These notions are based on C. H. Coombs's "unfolding analysis" discussed in his book, *A Theory*

TABLE 2. PREFERENCE ORDERS FOR NOMINEES*

1st Choice	2nd Choice	3rd Choice	Total	Blacks	White Democrats	White Independents	White Republicans
Humphrey	Nixon	Wallace	37%	93%	51%	25%	6%
Nixon	Humphrey	Wallace	37	4	19	42	70
Nixon	Wallace	Humphrey	9	0	7	12	13
Wallace	Nixon	Humphrey	9	0	8	15	8
Humphrey	Wallace	Nixon	5	3	10	3	1
Wallace	Humphrey	Nixon	3	0	5	3	2
			100%	100%	100%	100%	100%
		N =	997	112	344	265	251

* *Only the respondents giving different scores to the three nominees are included.*

more than two alternatives receive substantial numbers of last place choices, then multidimensionality of preferences is indicated. Inspection of Table 2 indicates that Nixon received only 8 percent of the last place scores compared to more than twice as many for Humphrey and many times more for Wallace. These results are basically consistent with a single dimension ranging from Humphrey at one end to Wallace at the other end with Nixon in the middle. This pattern would fit quite well with the journalistic left-right interpretation of the 1968 election. Given the known low level of ideological thought among the mass public, we would assume that this dimension is based more on specific issues than on a general liberal-conservative ideology. We shall consider later the nature of the issues leading to this left-right dimension.

The evidence, however, is not unanimously in favor of a unidimensional left-right interpretation. The two bottom rows of Table 2 run counter to the dimensional model which predicts that Nixon would receive no last place choices. Five percent of the respondents gave Humphrey their first choice and reached across the continuum to allot Wallace their second rather than last choice, although this involved bypassing the middle position on the scale. Also a full quarter of the 117 Wallace supporters in the sample gave Humphrey as their second choice, again violating the left-right dimension. While the overall level of fit is acceptable, we shall consider one further factor which may explain these deviations.

A model of partisan identification and its effects provides the additional explanatory power which is necessary. Party identifiers should give the lowest number of last place scores to the nominee of their party. Thus Democratic identifiers should give fewest last place choices to Humphrey, a prediction opposite to that of the left-right model. The two bottom rows of Table 2 are consistent with this notion of party identification for Democratic respondents but the two middle rows do not fit such an explanation. That fairly equal numbers of Democrats fall into each of these categories suggests that neither model is sufficient in itself.[9] In particular the 15 percent in each category would be a sizable level of error under any model. Predic-

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of Data (New York: John Wiley and Sons, 1964), pp. 80–121. The assumption is that an individual choosing among alternative stimuli orders them in terms of their distance from his point of maximum preference. As a result, on a continuum from left to right ordered A, B, C, people may give only the preference orders ABC, BAC, BCA, and CBA. Preference orders with the middle scale item, B, in the third choice position would violate this model so the ACB and CAB patterns would be nonexistent under the condition of perfect unidimensionality.

[9] An alternate explanation of the behavior of Democratic identifiers is that support for Wallace would not be considered defection from the Democratic party in the South given the peculiarities of Southern politics. This hypothesis yields the same prediction we have specified for the party identification model among Democrats. However, the evidence in favor of the prediction of the party identification model is even stronger among the Democratic North than the Democratic South, which suggests that the effects of party identification are more fundamental than are those of Southern politics. While some respondents may have viewed Wallace as a Democratic candidate because of his background, we choose, partly for ease of exposition, to regard his candidacy as separate from either major party. At a minimum there is no evidence that he was viewed together with the remaining Democratic candidates.

tions from party identification and the left-right theory postulated here are the same in the case of Republican identifiers, so no new information is gleaned from this source. The combined impact of party identification and the left-right model indeed yields a very good fit for Republican identifiers with only 3 percent giving Nixon their last choice. The correspondence of the choice patterns of Independent identifiers to left-right ideas is significant since party identification theory makes no particular predictions for their behavior. Given these results, the left-right dimension is a better ideological discriminator for Republicans and Independents than for Democratic identifiers. The purity of the dimension as an ideological discriminator is lessened for Democratic identifiers because of the conflict for some of them between their party identification and the left-right model. The overall story is basically a reading of two important factors at work—ideology and party, with the former having more weight in the interpretation but with the latter retaining considerable residual explanatory power.

Now that we have considered the preferences for the nominees, we wish to turn to the full spectrum of candidates. The mapping of the full candidate space necessitates a shift in analysis procedure. Analysis of the raw preference orders for the twelve candidates is beyond the reasonable limitations of simple inspection and hand analysis—there being 1306 respondents giving meaningful preference orders (different scores to at least two candidates) with a total of 1301 distinct preference orders being given. The alternative which we shall adopt is to analyze the intercorrelations of candidate ratings.[10] Candidates perceived in a similar fashion should have substantial positive correlations; those seen as quite dissimilar from one another should have sizable negative values. Correlation scores close to zero indicate an absence of shared perceptions of the candidates. The correlations among the three nominees, for example, are as follows: −.03 between Nixon and Wallace; −.18 between Humphrey and Nixon; and −.32 between Humphrey and Wallace. The relative magnitudes of these values indicate the very same locations for the nominees on the dimension as revealed in the preference orders of Table 2; the largest negative value, between Humphrey and Wallace, points to the basic opposition of the two and hence their placement at opposite ends of the continuum. Inspection of the correlation matrix of Table 3 provides a similar indication of the dimensions underlying the full set of data.

One pattern of interest in the matrix is the relationship of Wallace and LeMay to other candidates. Their correlations are low and often negative. In particular, the two are viewed in opposition to the Democratic candidates, and the generality of this statement could extend to most of the other political figures as well. This suggests the possibility of a Wallace-non-Wallace dimension, though such a dimension could also have partisan and/or issue overtones.

Another pattern in the correlation matrix stands out quite vividly. Candidates belonging to the same party generally have high positive correlations which implies that they are perceived in a similar vein. The four highest correlations involve presidential-vice-presidential teams (including Johnson and Humphrey) and the next eight highest also involve pairs of candidates from the same party. All the intra-party correlations are positive.

Conversely, candidates from different parties are usually viewed as dissimilar or unlike each other. The correlations between Republicans and Democrats tend to be negative, though lower in absolute value than the intra-party correlations. These results buttress the earlier notion of party identification as an important variable affecting the processes of political perception. However, an exception to this patterning stands out. McCarthy, Rockefeller, and Romney, those outside of their parties' "mainstreams," are somewhat correlated with each other—about as much so as with members of their own party. A common bond of popular perceptions may link these

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[10] The correlations measure the covariation in the ratings of candidate pairs. The average level of popularity of a candidate does not affect such covariation nor does the degree of dispersion in the scores given to a candidate. In particular, the covariation would not be altered by a linear transformation of the scores for a given candidate, such as a "bandwagon effect" which adds five degrees to every respondent's score for Nixon. All temporal effects need not involve simple linear transformations, but the covariation is less affected by such matters than the preference orders would be. Additionally, the use of the covariation measure may affect the distance between a pair of candidates in a spatial representation. A standard unfolding analysis would locate Nixon and Humphrey, for example, very near one another in a geometric space since large numbers of respondents rated both high. However, their correlation is actually a negative value, −.18. This indicates that the higher a respondent rated one of them, the lower he tended to rate the other; thus, the two belong in opposite parts of the space.

TABLE 3. CORRELATIONS OF CANDIDATE THERMOMETERS*

	H	M	K	J	Mc	Rk	Rm	A	N	Rg	L	W
Humphrey (H)	x											
Muskie (M)	.58	x										
Kennedy (K)	.53	.43	x									
Johnson (J)	.70	.46	.47	x								
McCarthy (Mc)	.25	.29	.36	.13	x							
Rockefeller (Rk)	.17	.27	.24	.16	.33	x						
Romney (Rm)	.17	.24	.26	.24	.33	.33	x					
Agnew (A)	−.10	−.03	−.01	−.04	.12	.14	.34	x				
Nixon (N)	−.18	−.09	−.13	−.09	.08	.13	.33	.60	x			
Reagan (Rg)	−.19	−.07	−.10	−.09	.09	.19	.31	.44	.41	x		
Lemay (L)	−.21	−.16	−.09	−.09	−.02	−.04	.10	.30	.11	.28	x	
Wallace (W)	−.32	−.26	−.22	−.23	−.13	−.14	−.06	.13	−.03	.20	.68	x

* All correlations are Pearson r's.

three men together. Kennedy is also associated with them, but he has much higher correlations with the members of his party.

A "left-right" ideological dimension or some specific issue manifestation of it might be underlying these patterns and might be intermixed with the partisan factor. The Democratic candidates tending to be more liberal than the Republicans might explain part of the patterning in the matrix as might the extreme position taken by Wallace. A left-right dimension within each party might also explain the relative correlation values for the separate parties. The bipartisan correlations of McCarthy, Rockefeller, and Romney (and, to some extent, Kennedy with them) also suggest an ideological or issue interpretation.

The final comment to be made about the structure of the correlation matrix is that four clusters of candidates are evident. The two American Independent Party candidates form one cluster. Three of the five Republicans—Agnew, Nixon, and Reagan—have their highest intercorrelations with one another as do four of the five Democrats—Humphrey, Muskie, Kennedy, and Johnson. There also appears to be a weak bipartisan cluster involving McCarthy, Rockefeller, and Romney.

In general, the existence of discernible clusters within the correlation matrix indicates an important degree of structuring in this body of data. The relations within and between clusters, however, are difficult to gauge from inspection of the matrix. The fundamental role of party in organizing the correlations is evident from even the most general look at the matrix and there is further evidence of a left-right dimension. However, the interrelationships between these dimensions cannot be found by simple inspection of the matrix. Closer inspection of the candidate space requires the use of a more sophisticated procedure for dimensional analysis.

We shall employ nonmetric multidimensional scaling[11] to obtain the dimensional solution which best explains the correlation patterns of Table 3. A nonmetric technique makes weaker assumptions than does factor analysis; it requires that only the ordering of the correlation values be considered as meaningful data and not the exact values themselves.[12] While making less stringent assumptions, such nonmetric techniques have been shown to yield fairly unique solutions. Multidimensional scaling interprets the correlations as monotonic with distances—the closer to +1.00 the correlation between two items (candidates), the closer together should be their corresponding points in a geometric representation. A "goodness-of-fit" measure, called "stress," is calculated to indicate the extent to which the best solution achieved in a given number of dimensions satisfies a monotonic fit with the data. This stress value is at a minimum for the correct solution and increases sharply in value as the number of dimensions being used is cut too far below the correct number. The analysis of the correlation matrix of Table 3 yields a "fair" solution in one dimension with a stress of .108 and a "good" solution in two dimensions with a stress of .050.[13] We shall look at these two solutions, one at a time, in order to see what explanatory power each offers.

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[11] See J. B. Kruskal, "Multidimensional Scaling by Optimizing Goodness of Fit to a Nonmetric Hypothesis," *Psychometrika,* 29 (March 1964), 1–27; J. B. Kruskal, "Nonmetric Multidimensional Scaling: A Numerical Method," *Psychometrika,* 29 (June 1964), 115–130. Also, L. Guttman, "A General Nonmetric Technique for Finding the Smallest Coordinate Space for a Configuration of Points," *Psychometrika,* 33 (December 1968), 469–506; J. C. Lingoes, "An IBM-7090 Program for Guttman-Lingoes Smallest Space Analysis—I," *Behavioral Science,* 10 (April 1965), 183–184; R. N. Shepard, "Metric Structures in Ordinal Data," *Journal of Mathematical Psychology,* 3 (July 1966), 287–315.

[12] There are several reasons why we have more faith in the order of the correlation values than in their exact magnitudes. First, individuals tended to restrict their responses to the nine scores cited on the thermometer card rather than using the full range provided by the thermometer analogy. Ordinal correlation values on such a nine point scale did

Figure 1 shows the best unidimensional solution which could be obtained.[14] Note first the partisan separation it provides. The Democratic candidates are at one end of the dimension, the American Independent Party candidates are at the other end, and the Republicans in between. Thus the partisan separation does not correspond to the usual party identification scale with Democrats and Republicans at opposite ends but rather parallels the order of the three nominees on a left-right dimension. The dimension of Figure 1 also distinguishes the clusters found in the correlation matrix. Humphrey, Johnson, Kennedy, and Muskie form one tight cluster and Agnew, Nixon, and Reagan form a second. McCarthy, Rockefeller, and Romney constitute an intermediate cluster between the two major presidential sets, similar to the pattern displayed in the correlation matrix of Table 3. McCarthy is closest to the Democrats, Romney to the Republicans, and Rockefeller midway between them. Ideologically these candidates are as liberal as the Democratic cluster in some respects though less liberal in others. The reasons for the central location of these candidates will be formulated more precisely when we expand the scope of this analysis to include various political attitude measures.

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not equal the correlation values earlier obtained, but the crucial point is that the order of such values was virtually identical for the two types of coefficients (Spearman's rho = .99). Second, some respondents may have given low scores to two candidates for opposite reasons—such as one candidate being too far to the left and the other too far to the right to satisfy the respondent. Giving similar low ratings to a pair of candidates adds to their correlation, even when the respondents involved actually saw the two candidates as quite distant from one another. This has little effect on correlations of candidates near one another in the space, but it may artificially increase the correlations between distant candidates. As a result, the negative correlations and some of the low positive correlations may be higher (in the direction of +1.0) than they should be, though the order of the correlations should be substantially unaffected. Third, all respondents did not necessarily translate the same feelings toward the candidates into the same thermometer values. Individuals could have different response set tendencies—some preferring to give candidates high scores and others tending to give them low scores, a result which would give an artificial positive boost to the correlation of any particular pair of candidates. Such slippage between a person's actual feelings and his verbal scoring of the candidates makes our correlations more positive (or less negative) than they should be. (A detailed proof of this regularity is beyond the scope of this paper.) One way to correct for this effect would be to compute correlations based on each individual's deviation scores from his mean; this, however, would destroy the entire meaningfulness of the thermometer scale and its "anchors" of 0, 50, and 100 degrees. The values obtained from such an operation would be different from our correlation values, but again the order of the two sets of correlations would be essentially similar. (In fact, the Spearman rho coefficient between the original correlations and those obtained by use of such deviation scores is .96, a value which is very high but which does permit some mismatch in the ordering of correlation values for given pairings of candidates.)

An additional consideration motivating the use of a nonmetric technique over factor analysis has to do with the proven tendency of the latter to overestimate the dimensionality of data of the type used here (C. H. Coombs, *A Theory of Data,* Ch. 8). Indeed some exploration with factor analysis on the thermometer data showed that it was supplying one more dimension than was uncovered by our use of a multidimensional scaling algorithm.

[13] The terms used are those suggested by Kruskal for the evaluation of various stress values.

[14] The exact details of such a solution should not be overinterpreted. Adjacent points, such as Johnson, Kennedy, and Muskie, might switch positions with one another if alternative assumptions had been made in the analysis. Thus small differences in the solution space should not be given too much credence. However, the gross features of the structure of the space—particularly clusters of points in that space—are generally invariant under the uncertainties governing this analysis.

INTERVIEWER: TAKE SOME TIME TO EXPLAIN HOW THE THERMOMETER WORKS, SHOWING R THE WAY IN WHICH THE DEGREE LABELS CAN HELP HIM LOCATE AN INDIVIDUAL, SUCH AS GEORGE WALLACE.

	Rating		*Rating*
a. George Wallace	________	g. Lyndon Johnson	________
b. Hubert Humphrey	________	h. George Romney	________
c. Richard Nixon	________	i. Robert Kennedy	________
d. Eugene McCarthy	________	j. Edmund Muskie	________
e. Ronald Reagan	________	k. Spiro Agnew	________
f. Nelson Rockefeller	________	l. Curtis LeMay	________

CARD #5

"FEELING" THERMOMETER

WARM	100° ____	Very warm or favorable feeling for candidate
	85° ____	Good warm or favorable feeling for candidate
	70° ____	Fairly warm or favorable feeling for candidate
	60° ____	A bit more warm or favorable than cold feeling
	50° ____	No feeling at all for candidate
	40° ____	A bit more cold or unfavorable feeling
	30° ____	Fairly cold or unfavorable feeling
	15° ____	Quite cold or unfavorable feeling
COLD	0° ____	Very cold or unfavorable feeling

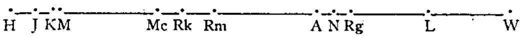

FIGURE 1. A Unidimensional View of the Candidate Space.

While Figure 1 provides the best fitting unidimensional ordering, we do not regard the candidate space as essentially unidimensional. The unidimensional solution distorts several of the relationships between the candidates. In particular the rule of monotonicity between the correlations and the corresponding interpoint distances demands even greater separation between Nixon and Wallace. It is useful to consider the ordering of the candidates on different possible continua. Nixon would be at the opposite end from the Democrats on a party identification dimension whereas Wallace alone would be at the opposite end from the Democrats on an issue continuum such as civil rights. Further separation between Nixon and Wallace is needed if both of these orderings are to co-exist.

The solution in two dimensions is shown in Figure 2. The four clusters of candidates are again evident and one can still separate the various parties in the space. The added dimension resolves some of the discrepancies in the one dimensional solution. The axes chosen for Figure 2 are intended to simplify the interpretation of this solution.[15] The horizontal dimension runs

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[15] In technical terms, we have employed a varimax rotation around the centroid of the space in order to approximate a simple structure solution. Multidimensional scaling solutions can be rotated

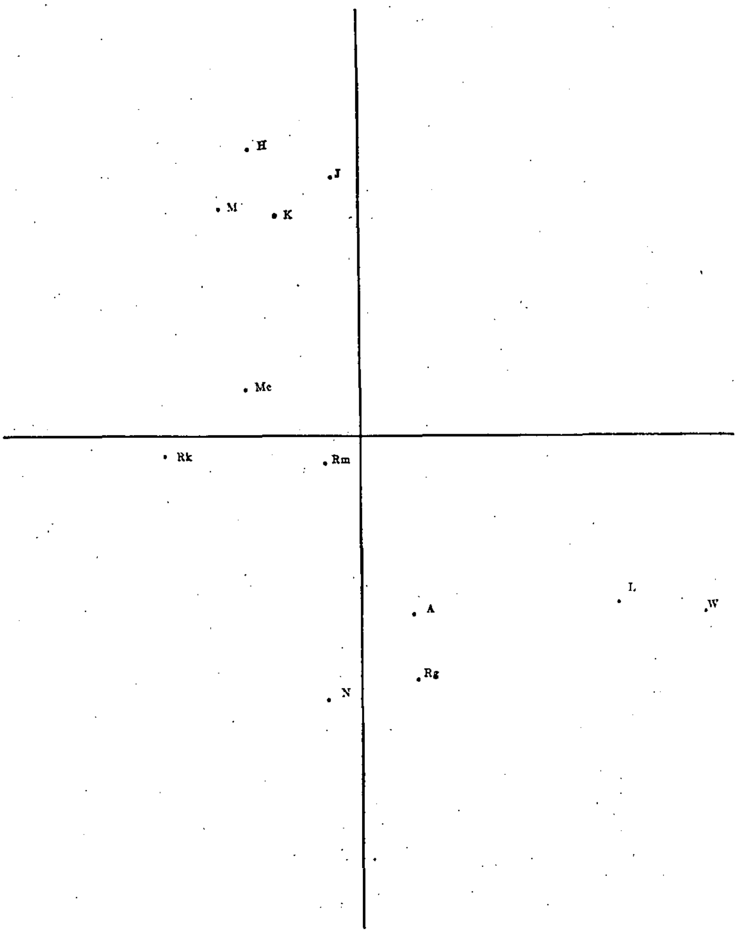

FIGURE 2. The Candidate Space in Two Dimensions.

from the Democrats and Rockefeller to Wallace. This ordering seems to correspond roughly to a left-right pattern or even to a Wallace-non-Wallace dimension. The vertical dimension runs from the Democratic candidates to the American Independent and Republican Party candidates. The ordering here might correspond roughly with party identification and attitudes on social welfare policy or government power more generally. We shall not justify the exact ordering on these axes but will further explore this question in the next section.[16]

The solutions we have seen do no great damage to our intuitive views of the candidates, but

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freely because the choice of axes in the multidimensional space is arbitrary. The arbitrary determination of the axes suggests that the overall structure of the space should be given the most emphasis or, alternatively, the relation of the candidate items to validating attitude items located in the same space should be stressed.

[16] The solution in Figure 2 still portrays the relationships between the parties more accurately than it portrays those within the parties. In particular it understates the distance between McCarthy and Johnson. The three dimensional solution resolves these remaining discrepancies, with the third dimension providing separation within each party. This dimension divides Johnson from Kennedy and McCarthy among the Democrats and divides Nixon and Agnew from Rockefeller, Romney, and Reagan. In each case it separates the "middle-of-the-road" candidates in the party from the more liberal and conservative candidates. Those controlling their parties' organizations are divided from those who opposed their parties' establishments. While the three dimensional solution provides an "excellent" fit to the data (stress = .018), this third dimension yields very little explanatory power so we shall not consider it further.

TABLE 4. CORRELATIONS BETWEEN POLITICAL ATTITUDES

	Party	Full Employment	Gov't Power	Urban Unrest	Civil Rights	Chicago	Vietnam	Foreign Aid
Party identification—Democratic*	x							
Favor government guarantee of full employment	.20	x						
Federal government is not powerful enough	.29	.26	x					
Solve urban unrest by social solutions rather than force	.15	.24	.15	x				
Favor desegregation	.05	.17	.16	.26	x			
Too much force was used in Chicago	.05	.20	.06	.36	.34	x		
Favor withdrawal from Vietnam rather than escalation	.02	.16	.11	.29	.13	.24	x	
Favor foreign aid	.04	.13	.08	.12	.22	.22	.00	x

* Labels for the issue items indicate the direction of their scoring.

looking at the candidates by themselves provides us with precious few handles by which to interpret the dimensions of the space. Therefore we shall consider the relations of attitudes toward the candidates to attitudes on issues and parties as a means of further explaining the candidate dimensions. This will allow us to note the similarities in the ways in which respondents view candidates, issues, and parties and will simplify the interpretation of the dimensions of the candidate space itself.

III. CANDIDATES, ISSUES, AND PARTIES

Political attitudes were important in the 1968 election and, in addition, seemed to stress other issues than those emphasized in earlier decades. The electorate of the 1960's was concerned with problems of the cities, civil rights, Vietnam, protest, and law and order.[17] Civil rights was not a new concern, but it now became associated with urgent new problems of domestic life and foreign affairs. The correlations in Table 4 show that this new issue cluster has a cohesive character of its own and is little related to partisan identification or to the classic social welfare and foreign policy areas. Past voting studies have highlighted the issues of social welfare and foreign policy, showing the two to be independent of each other and the former to be related to party identification.[18] Respondents still mentioned them in 1968 but they, by no means, had the salience and priority of the more contemporary focused issues. The other political orientation of concern, party identification, remained stable in 1968 compared to earlier years, but its lack of association with the new issue cluster may have dimmed its relevance in this election.

We shall now relate these attitudes directly to the full candidate space. We have chosen four items for closer analysis as being representative of the traditional party and social welfare areas and the new domestic and foreign concerns—the items being party identification and attitudes on full employment, solutions to urban unrest, and Vietnam, respectively.[19] Multidimensional scal-

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[17] Approximately three-quarters of the electorate listed one of these as the major problem facing the government when asked just before the 1968 election. See also Converse, *et al.*, "Continuity and Change in American Politics."

[18] Campbell, Converse, Miller, and Stokes, *The American Voter,* Ch. 9.

[19] The questions on urban unrest and Vietnam analyzed here and in Tables 6 and 7 below were devised by R. A. Brody, B. I. Page, S. Verba, and J. Laulicht.

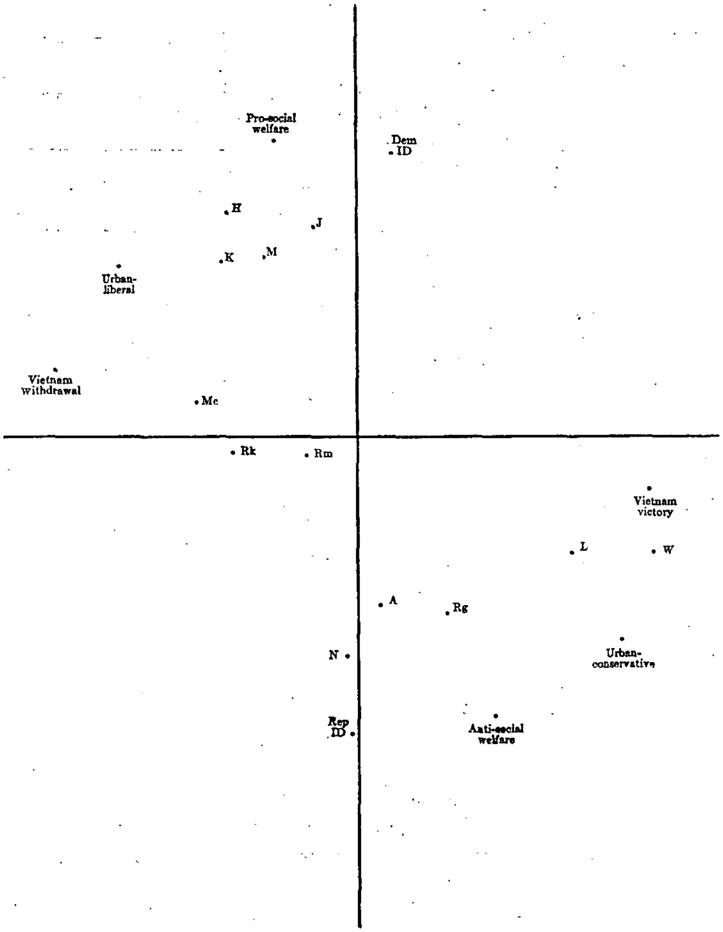

FIGURE 3. Candidates and Attitudes in Two Dimensional Space.

ing of these attitude items in both their original and reflected forms together with the candidate evaluations yields the solution shown in Figure 3.[20]

The vertical dimension of Figure 3 is basically partisan. The classic party identification item is associated with this dimension. Also the two partisan clusters have their highest loadings on it. The social welfare issue is quite close to this dimension—a result which fits well with the fact that party identification and social welfare concerns both grew out of the economic problems of the 1930's. Nixon is nearest to Republican identification while Reagan is closest to the conservative pole of social welfare, indicating that social welfare is tied to party though they are not completely identical.

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[20] The associated stress value is .106, indicating some difficulty in satisfying the monotonic constraints. The need to satisfy the additional relationships between the attitude items and the candidate ratings has affected somewhat the structure of the candidate space embedded in Figure 3, though we would regard this candidate space as being essentially similar to that of Figure 2. The attitude items were included in both their original and reflected forms in order to facilitate comparisons of their locations with respect to both liberal and conservative candidates. While the candidate ratings have a natural direction, the scoring of these attitude items is arbitrary. Therefore it makes sense to consider both possible directions for each item. Unlike some other analysis procedures, the multidimensional scaling model does not force the two poles of an item to be exactly opposite one another in the space, though we find this to be approximately true. If an item is related to a given axis, its alternative scorings would be at opposite ends of that axis. Both poles of an item unrelated to a dimension would project on approximately the same place on that dimension.

The horizontal dimension involves the more immediate problems of 1968. Vietnam attitudes are associated most strongly with this dimension and the urban unrest attitudes are also related to it. The American Independent Party candidates and McCarthy, Rockefeller, and Romney have their highest loadings here. The conservative ends of the Vietnam and urban unrest items are near Wallace and LeMay. The liberal end of Vietnam is nearest to McCarthy and Kennedy while the liberal end of urban unrest is closest to Kennedy and Humphrey. Respondents thus did differentiate within this set of issues, though overall perceptions of candidates on urban unrest and Vietnam are quite similar. Differentiation in respondents' perceptions was also evident in their contrasts of Wallace and LeMay with the rest of the candidates. While there have been previous indications of a Wallace-non-Wallace dimension, it is now apparent that such a dimension has issue bases and that some liberal candidates are also linked with this issue continuum.

The two dimensional space also comments on the relative positioning of the major party nominees. The basic opposition of Humphrey and Nixon along the vertical dimension points to the importance of party identification in molding evaluations of the candidates. Differences between them on the issue dimension were not considerable, but the bonds of traditional party identification kept them from moving toward a "tweedledee-tweedledum" position in the space. Even the intrusion of an issue dimension in 1968 did not eliminate the differences due to partisanship. In fact the partisan dimension is of somewhat greater importance than the issue dimension even in the full candidate space, though this should not be given too much emphasis since the measure of importance is considerably affected by the somewhat arbitrary selection of candidates included in the thermometer question.

The two dimensions of Figure 3 represent two basic political continua. The vertical axis corresponds to the partisan issues which divided the old left and the old right. The horizontal axis corresponds to the issues which divide the new left and the new right, issues which are not linked closely to the partisan attachments of an earlier generation. It is true that a very broad left-right dimension divides the issues and candidates in the upper-left corner of the diagram from those in the lower-right. Indeed such a continuum would correspond to the ordering of the candidates in Figure 1. However, the complexities of 1968 were too great for a good fit with a single left-right dimension. Equally important, issues apart from traditional partisan identification were critical in the determination of attitudes toward several of the candidates. The electorate did adapt to changing circumstances in its evaluation of the candidates. Indeed, the mapping of Figure 3 suggests considerable flexibility and sensitivity on the part of the electorate.

Nevertheless the data cannot indicate whether these dimensions are unique to the 1968 election or would have been found in a similar analysis of other recent presidential elections. The "new issues" dimension is most likely to be a recent development while the dimension similar to party identification probably has had a longer existence. Whether there is always a continuum relating to the issues of the day in addition to a long-term party identification dimension cannot be answered by the analysis of a single election. The closeness of the social welfare and party identification items might mean that an "issues of the day" factor would not appear when an election is fought along traditional social welfare lines. The issues of the day might form the basis of candidacies in any election, though we would regard the sharp dichotomization of the dimensions in this election as indicating that more fundamental concerns were at work in 1968.

The dimensional analysis has shown the critical roles of parties and issues in molding popular perceptions of the candidates in 1968. The impact of these factors on the specific candidates must be detailed as must the conditions governing their impact. The two factors could work together or separately, depending on the circumstances of the election year and its cast of leading characters. We shall now concentrate on how these factors operate on each of our twelve candidates and how they interrelate in their effects. We shall later specify the probable conditions under which the different dimensions of candidate evaluation are used, leading to a working knowledge of the formation of candidate evaluations.

The first factor of importance is party. Our suggestion that it is a crucial variable is not exactly novel. Past voting studies have shown that specific perceptions of the nominees are often expressed in terms of party ties. The authors of *The American Voter* have particularly detailed the respondents' likes and dislikes of the nominees in these terms, most notably in the Eisenhower-Stevenson elections of 1952 and 1956. They found that party provided an important basis for the evaluations of these two men—perhaps initially in 1952 because it was one of the few cues available to evaluate such new personalities on the national political scene. They also traced the impact of party on reactions to the personal characteristics of the two nominees and found the expected partisan slope, adherents of each party evaluating their party's candidate more favorably than did adherents of the opposite party.[21]

The theme of party influence was carried further in the literature by Converse and Dupeux as they probed the complexities of the Eisenhower case.[22] They point out that Eisenhower was long seen apart from the party system and that Democratic leaders, at least at one time, hoped he would run for office as the candidate of their party. Reaction to him in the 1952 campaign was influenced by his eventual choice of the Republican banner. Yet Converse and Dupeux argue that there was no reason to feel that admiration for him had previously followed partisan lines and if Eisenhower had chosen the Democratic party, "we may assume the relationship would have rotated in the opposite direction: strong Republicans would have decided they disliked Eisenhower."[23] Party was thus seen as a strong and inexorable influence on the perceptions of the candidates.

Given such research, one should expect that party would be a major orientation to the candidates in 1968. After all, most of the candidates had long backgrounds in partisan politics and were known as national political figures prior to the 1968 campaign. People like Humphrey, Nixon, and Johnson were considered leaders of their parties and had all held national elective office. Kennedy was well-known as a key participant in his brother's Democratic administration, for being a prominent Democratic senator, and for taking his campaign in 1968 directly to the people. McCarthy, too, was a Democratic office-holder and conducted a quite visible campaign in his party's primaries. On the Republican side, Rockefeller had participated in national campaigns for his party's nomination since 1960 and Romney, also a governor of a large state, was considered by many to be the front-runner for his party's top prize until the start of the 1968 primary season. Reagan did not mount a national campaign but received considerable publicity as a result of his political success in California. Muskie and Agnew were less well-known nationally but were naturally tied to and identified with their party's presidential ticket, awareness of them increasing as the campaign progressed. Only Wallace and LeMay provided a contrast to this general theme in publicly disavowing the two party system and running a third party campaign. With that exception, the candidates of 1968 had direct partisan connections and were generally familiar in that guise. In a very real sense, the party cue was available longer to these men than to either Eisenhower or Stevenson in 1952. Whether they actually were so perceived, however, is a question to which we now turn our attention.

Table 5 suggests the effects of party identification on the candidate evaluations. Partisan slopes are clearly evident for several of the candidates. Strong Democrats gave the highest mean scores to Humphrey, Muskie, Kennedy, and Johnson while strong Republicans gave them the lowest mean scores. This pattern is exactly reversed for Agnew, Nixon, and Reagan. The difference in means between the strong identifier groups for these candidates is 18–37 percent, indicating an effect of considerable magnitude. These seven partisan candidates represent a mixture of those who have been on the national scene the longest and shortest times. In some instances the candidates have long been identified with the parties while in other cases party is one of the few available cues for candidate evaulation. The patterns shown for the latter type of candidate are similar to those found for Eisenhower and Stevenson. But while all seven of these political personalities had sizable party slopes, there is a marked tendency in Table 5 for candidates having been on the national scene the longer time and candidates associated with higher partisan office to have the larger partisan slopes in the group.

A marked departure from partisan guidelines appears for the remaining candidates. Wallace and LeMay, in particular, were seen in relatively non-partisan terms. Their levels of strength, when weak and strong identifiers of each party are combined, are about the same among Democrats and Republicans, indicating that on balance they were not seen as Democrats regardless of Wallace's past partisan ties. Being third party candidates, it is also not surprising that support for them varied inversely with strength of party identification and that, as a result, their greatest backing came from among those not identifying with either major party. There may even have been a shift of Wallace supporters into the ranks of the Inde-

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[21] Campbell, Converse, Miller, and Stokes, *The American Voter*, pp. 128–131.

[22] P. E. Converse and G. Dupeux, "De Gaulle and Eisenhower: The Public Image of the Victorious General," translated in A. Campbell, P. E. Converse, W. E. Miller, and D. E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, 1966), pp. 292–345.

[23] *Ibid.*, p. 325.

TABLE 5. MEAN SCORES OF CANDIDATES BY PARTY IDENTIFICATION

	Strong Democrats	Weak Democrats	Independents	Weak Republicans	Strong Republicans	Eta*
Humphrey	81.1	66.0	54.5	50.8	43.6	.46
Muskie	73.8	60.8	58.4	55.5	51.8	.31
Kennedy	81.9	72.7	65.8	61.2	55.5	.32
Johnson	75.7	63.3	51.5	48.0	42.3	.43
McCarthy	55.7	53.8	55.2	56.9	50.6	.08
Rockefeller	54.5	51.7	52.6	58.0	54.0	.09
Romney	49.5	47.4	48.3	49.1	53.8	.10
Agnew	43.8	48.3	50.3	56.0	61.6	.25
Nixon	54.9	61.3	66.8	77.1	84.2	.40
Reagan	41.2	46.9	50.2	53.7	60.2	.25
LeMay	28.4	37.6	38.5	34.6	35.0	.14
Wallace	24.3	35.2	35.5	32.1	23.6	.16
N's	238–267	298–331	356–381	163–180	125–132	1192–1291

* The eta statistic indicates the degree of relationship between the independent variable (party identification) and the candidate's ratings.

pendents, although the small differences observed in Table 5 suggest that only a few members of the electorate probably made such a change. The other candidates—McCarthy, Rockefeller, and Romney—received approximately equal backing from all classes of identifiers. This means that their appeal was not strongly partisan and consequently did not reap the usual advantages of partisan support. This finding is more unusual than the Wallace-LeMay pattern since the three had definite partisan backgrounds and were identified with the race for their parties' nominations. What remains to be considered is the bases of support which these five candidates employed instead of the usual partisan appeal. Conversely, it will also be important to clarify why the other candidates were seen as "party people" and to determine in what other ways they were viewed.

To explore these questions further, we have examined the relationships between the scores given to the candidates and two of the new issue items. The first item had the respondent indicate on a seven point scale what he considered to be "the best way to deal with the problem of urban unrest and rioting." The scale ranged from solving the underlying problems of poverty and unemployment at one end to using all available force at the other. This "urban unrest" question evoked some of the respondent's basic feelings toward the subjects of law and order, militancy, civil rights, and social welfare. Table 6 shows the mean scores given the candidates by attitudes on urban unrest and enables us to look for "issue slopes" in the same manner that we looked for partisan slopes in the previous table.[24]

The data in Table 6 indicate that some of the more partisan candidates were also seen in issue terms. For instance, Humphrey, Kennedy, and Johnson were evaluated more highly by those in favor of solving the problems of our domestic life by social justice instead of law and order. Muskie also followed suit, but not as much so as his Democratic colleagues. Of the partisan Republicans, Reagan was liked most by those advocating the use of all available force, though the difference is not a sizable one. Feelings toward Nixon and Agnew also tended to be more favorable on the "law and order" side, but their strength is nearly constant across the continuum. Such slopes show that the strength of the issue factor varied for the partisan candidates, although each candidate was evaluated higher on one side of the continuum than the other. A comparsion of these results with Table 5 reveals that the urban unrest issue is not as important as party in its effects on this set of candidate ratings.[25]

TABLE 6. MEAN SCORES OF CANDIDATES BY ATTITUDES ON URBAN UNREST

	Solve problems of poverty and unemployment			Use all available force		Eta
Humphrey	71.5	67.7	59.1	51.7	50.2	.32
Muskie	66.2	66.8	60.4	55.6	54.2	.22
Kennedy	78.6	72.3	67.5	59.3	61.0	.28
Johnson	64.8	62.3	58.7	49.8	48.9	.24
McCarthy	60.0	58.3	52.3	52.0	50.2	.18
Rockefeller	56.0	56.6	54.7	50.6	49.6	.12
Romney	48.9	52.6	51.6	48.3	43.9	.16
Agnew	48.6	48.5	51.3	52.6	51.4	.07
Nixon	63.2	63.9	68.8	69.9	66.8	.11
Reagan	43.2	47.4	51.3	51.4	54.3	.19
LeMay	26.3	30.5	35.1	37.9	47.7	.29
Wallace	18.2	24.4	28.9	38.5	51.1	.39
N's	358–389	124–129	343–369	115–126	224–249	1173–1261

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[24] The seven point scales on this and the next issue were collapsed into five point scales by combining the two extreme positions at each end.

[25] In order to gauge the relative importance of these effects, it is necessary to consider the slopes, the distribution of cases on the issue and party variables, and the curvilinear tendencies for some of the candidates. The eta coefficient takes all of these matters into account and, hence, forms the basis for our judgments of relative importance. When squared, it relates the proportion of variance in a candidate's ratings explained by a given factor. Eta values for two variables can be com-

TABLE 7. MEAN SCORES OF CANDIDATES BY ATTITUDES ON VIETNAM

	Immediate Withdrawal			Complete Military Victory		Eta
Humphrey	64.4	65.8	64.8	63.1	53.3	.19
Muskie	62.5	63.5	63.0	63.0	57.0	.12
Kennedy	76.3	73.2	70.6	67.2	62.6	.20
Johnson	57.0	60.8	62.6	58.8	53.3	.14
McCarthy	61.5	58.9	55.6	52.5	49.6	.20
Rockefeller	54.0	59.1	55.2	55.8	50.3	.12
Romney	46.7	50.2	51.5	51.3	47.0	.12
Agnew	46.8	49.1	50.8	51.6	52.4	.10
Nixon	61.9	69.1	67.1	68.5	67.0	.11
Reagan	43.0	47.6	49.4	51.8	52.4	.16
LeMay	26.4	28.4	31.8	33.9	45.0	.27
Wallace	21.8	20.5	25.7	30.8	45.1	.31
N's	246–275	95–101	346–369	125–133	336–357	1152–1234

Perhaps the most interesting patterns in the table concern the candidates lacking partisan ties. Issue colorations were evident for most of these individuals. Wallace and LeMay, in particular, were supported by those wanting all available force with a strong slope toward the opposite end. The difference in means for Wallace between the two extreme categories was 33 percent, the largest such difference in the table. McCarthy, by contrast, was seen as a liberal with a definite tendency existing for people to evaluate him, as Humphrey, Kennedy, and Johnson, on the social justice end of the urban continuum. Rockefeller and Romney also did best on the social justice side, though the differences are not large which may be due, in part, to their lower level of saliency among the public. The scores given to these five candidates have a greater relation to this dimension than to party identification. The overall public ranking of these and the other candidates along this dimension fits very well with the interpretations given by political commentators. In some cases we are not dealing with large effects, but there is evidence that this dimension was quite salient to the public and that they could accurately locate the candidates on it.

The other overriding issue in 1968 was certainly the Vietnam war. Following a format similar to the urban unrest question, respondents were asked what action they felt the United States should take in Vietnam. The choices ranged from immediate withdrawal at one end to winning a complete military victory at the other. Table 7 shows the mean scores given the candidates by attitudes on Vietnam. The war generally affected ratings of the partisan candidates less than did either party or urban issues. Still, some differences on Vietnam did appear among the partisans with higher ratings for Kennedy among the "doves" and somewhat higher scores for Reagan and Agnew among the "hawks." Humphrey and, to a lesser extent, Muskie fared better on the dove side, but their ratings fell off at the extreme. Johnson manifested another pattern—those in the middle of the scale liked him most and then his support trailed off as one moves to the more extreme categories. This fits well with Johnson's "middle-of-the-road" handling of the war and indeed the two extremes of this scale were intended as alternatives to his policies. Nixon's level of support varied little with Vietnam policy except among strong doves who tended to rate him lower. His announcement of a "plan to end the war" without indicating the nature of that plan appears to have hurt him only among doves.

Vietnam attitudes tend to have more impact on the ratings of the remaining candidates than party and are almost on a par with those on the urban issue. Leading off this parade of effects are Wallace and LeMay who are once again found at one of the extremities of the continuum. The two definitely show the strongest issue slopes on the "hawkish" side of the Vietnam question, pointing up the way in which they embodied the issue scene of 1968. Evaluations of McCarthy were also affected by attitudes on the war with the Senator receiving his greatest support among those wishing immediate withdrawal from Vietnam. There is every indication here that his candidacy was visible and that he did succeed in tying it to the war issue. Rockefeller's and Romney's patterns are more ambiguous, again possibly due to their lower level of saliency. Romney's best standing was among those favoring "middle-of-the-road" policies toward the war, possibly suggesting that he experienced difficulty in communicating his war position to the public. Rockefeller's virtual silence on the war may explain the irregularities in his pattern of support, though he definitely did garner greater backing among doves. Basically the data fit usual statements of the twelve candidates' policies quite well. The effects of Vietnam attitudes tend to be smaller than those associated with urban problems, but this may well be explained by smaller differences between the candidates and less clarity on their part in the statement of exact positions on the war.

The data presented here show that some of the candidates not seen in partisan terms are instead associated with the issues of the times. In order for a candidate to become strongly identi-

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pared to ascertain which factor, issue or party is the more important for the candidate of concern.

fied with an issue position, it is necessary for him to be salient to the public and for him to take a definite stand on the issue. Wallace and LeMay provide the strongest contrasts in data patternings since they were not partisan candidates and did become strongly identified with the important issues. McCarthy, Rockefeller, and Romney also were differentiated on issue bases, though not as strongly so as Wallace and LeMay because of their lower salience and their less definite positions on some issues. While an elite audience would generally recognize sharper issue positions for these three candidates, their limited salience to the mass public dampened public perceptions of these positions so that only relatively mild reverberations of their issue stands are evident in the data.

Among the partisan candidates, issues were not always relevant, but some contrasts do appear. The Democratic hard core—Kennedy, Johnson, Humphrey, and Muskie—registered moderate issue effects, particularly on solutions to urban problems. Reagan alone among the Republican candidates was seen in fairly distinct issue terms. But generally assessments of the partisan candidates were not necessarily based on current issues whereas the evaulations of the non-party candidates were related to such issues. That issues can be of the same order of importance as party in determining attitudes toward candidates, even candidates for the nominations of the major parties, is significant.

The impact of party and issue factors on perceptions of the candidates has been noted and detailed. The next step is to suggest conditions under which these two factors operate in the molding of candidate orientation. Party seems to be a useful cue for candidate evaluation when the individual is a new candidate without well-known policy stands, as in the case of Eisenhower, Stevenson, Agnew, and Muskie. Party will also loom important as a determinant of ratings when the candidate is a well-known national leader of his party, e.g., Humphrey, Nixon, Johnson, and Kennedy. The candidates of the major parties may still be perceived in issue as well as party terms; Kennedy and Reagan provide examples, though we find them viewed significantly more in the latter than in the former vein. Candidacies based mainly on issues are also possible, even in the major parties. A candidate without a decidedly partisan national reputation may distinguish himself on an issue basis, with little regard to conventional party lines. Wallace and LeMay demonstrate this possibility as third party candidates. McCarthy and, to a lesser extent, Rockefeller and Romney also tend to exhibit the patterning of an issue-based candidate. The conditions making such issue candidacies viable remain to be considered.

IV. AN ERA OF INCREASING IDEOLOGICAL FOCUS

Two ideal types of "ideological focus" have been distinguished by Donald E. Stokes. Some periods of time can be characterized by "strong ideological focus" with political controversy "focused on a single, stable issue domain which presents an ordered-dimension that is perceived in common terms by leaders and followers." By contrast, a period of "weak ideological focus" would be one in which political conflict is "diffused over a number of changing issue concerns which rarely present position-dimensions and which are perceived in different ways by different political actors."[26] The nature of candidacies is basically dependent on the degree of ideological focus; viable issue candidacies require the sharp issue conflicts of periods of strong ideological focus. This election must be viewed in the broader perspective of the nature of party conflict in the last generation in order to determine the impetus for issue candidacies in 1968. This will lead us to an ultimate consideration of the future shape of party competition.

The contemporary period has been typified by the conditions of weak ideological focus and this is particularly true of the 1950's. While there were "issues" of a sort in that decade, they tended not to be position issues. One doubts whether the public perceived the Taft-Eisenhower contest in 1952, the Stevenson, Kefauver, Russell, Harriman, and Barkley contests of 1952 and 1956, or the Eisenhower-Stevenson elections in strong issue terms. In particular Eisenhower and Stevenson did not emerge on the national political scene as the embodiments of strong issue positions. In periods of weak ideological focus, one expects that the candidates will be more party-based than issue-based. The candidates, particularly the new candidates without a partisan reputation, will be more positively evaluated by the identifiers of their own party since there are few competitive cues available.

The degree of ideological focus of American politics has been low since the period of the New Deal. "Then, more than now, the intervention of government in the domestic economy and related social problems provided a position-dimension that could organize the competition of parties and the motivation of electors."[27] What is remarkable is that the social welfare questions which realigned the parties in the 1930's still constitute the basis of party identification regardless of the many changes in our life since then. The stability of partisan loyalties is such that it does not change until the circumstances of the day force such a change.

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[26] D. E. Stokes, "Spatial Models of Party Competition," p. 376. "Position-dimensions" involve dimensions of conflict on which political actors—voters and parties—can and do take different policy stands.

The stability of partisanship at the mass level has a parallel among the party leaders. The electorate maintains its loyalties, in part, because the party leaders keep their doctrines relatively fixed. The parties originally became differentiated with respect to certain issues, such as social welfare. The differences on these issues are maintained by the parties in order to keep their underlying group support, though these differences may be muted in order to gain electoral advantage.

It is in periods of strong ideological focus that this stability is most seriously threatened. A set of issues may accumulate with little regard to conventional party lines. The parties tend to avoid involvement with new position issues for fear of losing their base of support, instead maneuvering to establish somewhat similar positions on these issues. This allows minor parties to take advantage of the new issues, at least for the short run. If the new position issues permit the minor parties to make a noticeable dent into the normal vote of the major parties and if furthermore these issues do not show signs of receding, the major parties shift their stands on these issues. The resulting changes in group loyalties betoken a realignment of the parties.[28] Those first joining the electorate during a period of strong ideological focus are less tied to traditional party lines and are often most affected by the new issues; thus the addition of young voters to the system can help provide the momentum needed for a realignment. Issue-based candidacies are more likely in a period of strong ideological focus; the actual nomination of an issue-based candidate by a major party can provide the final spur needed for the realignment to take place. Our political system is stable because few issues are of the magnitude necessary to cause a realignment, but they are not totally absent from the political scene.

Civil rights is one issue which could cause a realignment. It formed the basis of a regional third-party movement in 1948, but no lasting change resulted. The Supreme Court opinions of the 1950's increased the immediacy of the issue. The major parties first became actively involved in the issue in the 1964 election, but the basic problem could still be geographically isolated. By the 1968 election, however, civil rights was a national problem. Furthermore, the civil rights problem fit into a more general syndrome which also included riots in the urban ghetto, campus unrest, protests against the Vietnam war, disorders on the streets of Chicago during the Democratic convention, and the general "law and order" theme. The establishment candidates in the major parties did not take sharply different positions on these issues, basing their appeals instead on conventional issues and party ties. Neither the Republicans nor the Democrats moved to a position on the new issues which would satisfy the extremists on either side, though their differences on the core civil rights problem were sufficient for black Americans to have no doubt as to their direction.

The echoes of these developments are evident in our data. We have found that a new set of issues has emerged, quite distinct from both social welfare and party identification. The civil rights issue of the previous decade provides the core of this new issue cluster, but further domestic and foreign problems are now associated with it. The independence of this new set of issues from the traditional concerns was particularly evident in the candidate/attitude space of Figure 3. Since a new position-dimension emerged at the mass level without the nominees of the major parties taking very different stands on it, a minor party emerged at one end of the dimension. Candidates associated with the liberal end of this dimension unsuccessfully contested for their parties' nominations, but chose not to take their fight to the electorate with a fourth party movement, at least not in 1968.

If our statement of the development of a new issue area independent of conventional party lines is correct, we are left speculating as to the shape of the political future. The new issues could recede in urgency by 1972. This could occur with reference to Vietnam, but seems less probable with regard to the other issues. It is always possible that the civil rights problem will become less urgent for a short period of time, but the long-term trend seems to be one of greater urgency. Protest, the complaints of youth, and the law and order theme are likely to become familiar parts of the political landscape. If indeed the issues do maintain their level of urgency, we may see some efforts toward party realignment by the 1972 election. Efforts toward reform of Democratic party procedures are already suggesting the feasibility of such a realignment.

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[27] *Ibid.*

[28] A classification of elections in terms of maintaining, deviating, and realigning elections is given in Campbell, Converse, Miller, and Stokes, *The American Voter*, pp. 531–538.

The changes in the political horizon have a particularly important effect on the youth. Many have been directly affected by the new issues and all have witnessed a more vigorous political climate than that of the previous generation. Not having long participated in the political system, they tend to be less firmly identified with a party and less firmly committed to the parties than are their elders. The three party race of 1968 along with the new issue dimension and the issue candidacies may have further delayed the first real commitment to the party system for many young adults. As a result the pool of young voters who may enter the political system for the first time in the 1972 election includes more than the usual number of delayed entries. Additionally that pool will be larger than in most recent years because of the effects of the post-war "baby boom." That many of the new young voters will be veterans of the Vietnam conflict introduces another note of uncertainty. Taken together these elements point to a potential increase in the fluidity in the electoral system in 1972.

Thus the 1964 and 1968 elections could well constitute the prelude for a series of changes of a scope more vast than those to which voting studies have become accustomed. A deviating election such as 1968 may mark the end of a political era, though it would be too early to suggest the outline of a new one. In particular, the seeming stability of party identification may just mask an increasing irrelevance of traditional party ties during a period of growing ideological focus when some of those ties are becoming unhinged. This election was marked by events having theoretical importance—a new issue dimension developed, issue candidates emerged in the major parties, and a third party made a sizable showing. The voting patterns of significant groups in the electorate, particularly blacks, seem destined for a meaningful change from their patterns in the 1950's. Only time will tell whether a lasting reorientation of American politics occurs along these lines, but a considerable change in the panorama of American politics seems likely even if a full realignment is avoided.

There are two major dimensional antecedents of candidate evaluation. Party provides a basic dimensional antecedent, especially during periods of weak ideological focus when people unknown on the national political scene are nominated by their parties and also when the party leaders fight a rear-guard action against a realignment of the parties along the lines of the issues of the day. However, there is also room for issue dimensions, particularly in periods of strong ideological focus. Minor parties may grow up at the poles of such issue dimensions, though one would expect realignment of the major parties if those issue dimensions remain vigorous for any period of time. Party and issues thus provide two basic mechanisms of candidate evaluation. In 1968 we had both party-based candidates and issue-based candidates, a fact which in itself may be quite suggestive of the future of American politics.

APPENDIX

As you know, there were many people mentioned this past year as possible candidates for President by the political parties. We would like to get your feelings toward some of these people.

I have here a card (INTERVIEWER HANDS OVER CARD #5) on which there is something that looks like a thermometer. We call it a "feeling thermometer" because it measures your feelings toward these people. You probably remember that we used something like this in our earlier interview with you.

Here's how it works. If you don't feel particularly warm or cold toward a person, then you should place him in the middle of the thermometer, at the 50 degree mark.

If you have a warm feeling toward a person, or feel favorably toward him, you would give him a score somewhere between 50° and 100°, depending on how warm your feeling is toward that person.

On the other hand, if you don't feel very favorably toward a person—that is, if you don't care too much for him— then you would place him somewhere between 0° and 50°.

Of course, if you don't know too much about a person, just tell me and we'll go on to the next name.

Our first person is George Wallace. Where would you put him on the thermometer.

CORRELATES OF PUBLIC SENTIMENTS ABOUT WAR: LOCAL REFERENDA ON THE VIETNAM ISSUE

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Political science, by whatever definition of the discipline one might want to adopt, traditionally has been concerned with public opinion and participation on significant policy questions. Although the literature has become too vast for a complete enumeration of the varied contexts in which this research has been conducted, one issue that might rank high on a list of priorities for study—and yet has received somewhat less emphasis than other topics—is the subject of public attitudes toward war.

Perhaps this relative neglect has been promoted by a lack of opportunities for direct public participation in foreign policy decisions. Unlike most domestic issues, controversies over world problems have been relatively insulated from popular influence. Hence, research on the development of international conflict usually has devoted more attention to the statements and behavior of national leaders or key influentials than to public sentiments regarding war.[1]

In recent years, however, the bitter debate generated by the war in Vietnam has stimulated mounting interest in popular attitudes concerning military action.[2] The controversy has provoked both an unusual display of public disagreement about the war and a desire for basic changes in the policy-making process. Many persons not only have registered strong disapproval of American involvement in the Vietnam war, but they also have expressed an acute sense of frustration about their inability to affect the conduct of international relations. As a result, growing demands have emerged to permit expanded public access to critical decisions and to create increasingly democratic methods of formulating foreign policy.

The probable outcome of enhanced public participation in framing foreign policy decisions, however, has been difficult to assess. Since voters seldom have been able to exert a direct influence on foreign affairs, government officials—as well as anti-war leaders—have not been able to determine whether increased opportunities for the expression of public sentiments about international problems would produce demands for more conciliatory or more militaristic policies. As Robinson and Jacobson reported after a thorough summary of existing data on the Vietnam issue, "In spite of increasing inquiry and speculation about the psychological basis of war, we have few theories and fewer empirical studies of public support for any war—let alone the war in Vietnam."[3]

Perhaps one of the most perplexing problems in the analysis of public opinion on war has been a lack of clarity concerning the foreign policy attitudes of persons at different socioeconomic levels. Early research suggested that variables such as party or group influences and affiliations were more important than social class in determining foreign policy attitudes.[4] In addition, studies of voting behavior indicated that, in contrast to voters whose interest in foreign policy outweighed their interest in domestic problems, there was an increased prevalence of "status voting" among persons who were more concerned about domestic than foreign policy issues.[5] A careful investigation of survey data collected since World War II concluded that, in comparison with other attributes, "the social class variable seems to be less clearly and less strongly related to attitudes on Cold-War issues."[6]

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[1] See, however, Gabriel Almond, *The American People and Foreign Policy* (New York: Harcourt, Brace, 1950); James N. Rosenau (ed.), *Domestic Sources of Foreign Policy* (New York: The Free Press, 1967).

[2] See, for example, William A. Gamson and Andre Modigliani, "Knowledge and Foreign Policy Opinions: Some Models for Consideration," *Public Opinion Quarterly*, 30 (Summer 1966), 187–199; William C. Rogers, Barbara Stuhler, and Donald Koenig, "A Comparison of Informed and General Public Opinion on U. S. Foreign Policy," *Public Opinion Quarterly*, 31 (Summer 1967), 242–252. A comprehensive review of the literature relating to public opinion on issues of war and peace has been prepared by William Eckhardt and Theo. F. Lenz, "Factors of War/Peace Attitudes," *Peace Research Reviews*, 1 (October 1967), 1–114.

[3] John P. Robinson and Solomon G. Jacobson, "American Public Opinion About Vietnam," *Peace Research Society (International) Papers*, 10 (1968), 75.

[4] George Belknap and Angus Campbell, "Political Party Identification and Attitudes Toward Foreign Policy," *Public Opinion Quarterly*, 15 (Winter 1951–52), 601–623.

[5] Philip E. Converse, "The Shifting Role of Class in Political Attitudes and Behavior," in Eleanor E. Maccoby, Theodore M. Newcomb, and Eugene L. Hartley (eds.), *Readings in Social Psychology* (New York: Holt, Rinehart and Winston, 1958), pp. 396–

The association between socioeconomic status and attitudes toward the Vietnam war also has not been particularly clear. A major national survey of public opinion on the war in Vietnam, for example, reported that policy preferences on the Vietnam issue were not significantly related to accepted measures of social class such as income, education, and occupation.[7] Subsequent data analyses, however, have indicated that "differences by social status and age . . . seem more visible, with those of higher status and middle age most in favor of war policies."[8] In addition, two other national studies found that lower-class persons were more likely than middle- or upper-status respondents to support "moderate" or "conciliatory" policies in both the Vietnam and Korean conflict[9] as well as to favor a de-escalation of the Vietnam war.[10]

Despite the latter evidence, many observers adopted the conventional belief that working-class segments of the population have failed to express strong opposition to the Vietnam war. The report prepared by Jerome Skolnick for the National Commission on the Causes and Prevention of Violence concluded that, for a variety of reasons, "rank-and-file American workingmen have not supported the peace movement."[11] The failure of working-class voters to make a conspicuous or major contribution to the anti-war protest movement, however, might not have reflected the actual distribution of public attitudes about the Vietnam issue. In fact, participation in mass demonstrations may have provided a less accurate measure of popular sources of support or opposition concerning the war than survey responses or voting behavior.[12]

Perhaps a major weakness of prior studies of public sentiments about war and peace has resulted from the relatively remote character of those controversies. Perceptions of complicated international issues often have been affected by feelings of patriotism and by the statements of respected national leaders.[13] In addition, investigations of foreign policy attitudes seldom have been linked to specific acts of political behavior or participation. Since many respondents may have become aware that their opinions will have less impact on international than on domestic issues, perhaps their replies have been shaped by special influences or constraints that have not affected policy preferences on other issues. Increased opportunities for direct popular participation in the development of foreign policy, therefore, could have a major effect on public attitudes toward war.

Perhaps the only means by which rank-and-file citizens can influence military actions directly, however, is through a popular referendum. In fact, this proposal was nearly adopted by Congress in 1937. The so-called "Ludlow Amendment," which would have granted the electorate "sole power by a national referendum to declare war or to engage in warfare overseas," was rejected in the House of Representatives by

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397; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley, 1960), p. 361.

[6] Milton J. Rosenberg, "Images in Relation to the Policy Process: American Public Opinion on Cold-War Issues," in Herbert C. Kelman (ed.), *International Behavior: A Social-Psychological Analysis* (New York: Holt, Rinehart and Winston, 1965), 317.

[7] Sidney Verba, Richard A. Brody, Edwin B. Parker, Norman H. Nie, Nelson W. Polsby, Paul Ekman, and Gordon S. Black, "Public Opinion and the War in Vietnam," this REVIEW, 61 (June 1967), 317–333.

[8] Robinson and Jacobson, "American Public Opinion About Vietnam," 67.

[9] Richard F. Hamilton, "A Research Note on the Mass Support for 'Tough' Military Initiatives," *American Sociological Review*, 33 (June 1968), 439–445; Richard F. Hamilton, "Le Fondement Populaire des Solutions Militaires 'Dures': Le Cas de la Chine en 1952," *Revue Française de Sociologie*, 10 (1969), 37–58.

[10] Martin Patchen, "Social Class and Foreign Policy Attitudes," unpublished paper presented at the annual meeting of the American Sociological Association, San Francisco, California, September, 1969.

[11] Jerome H. Skolnick, *The Politics of Protest* (New York: Ballantine Books, 1969), p. 58.

[12] The 1968 elections study conducted by the Survey Research Center indicated that white opposition to the Vietnam war was associated with disapproval rather than approval of anti-war demonstrations. See Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," this REVIEW, 63 (December 1969), 1087–1088.

[13] Eugene J. Rosi, "Mass and Attentive Opinion on Nuclear Weapons Tests and Fallout, 1954–1963," *Public Opinion Quarterly*, 29 (Summer 1965), 280–297; S. Putney and R. Middleton, "Student Acceptance or Rejection of War," *American Sociological Review*, 27 (October 1962), 655–666.

21 votes through intense pressure from the Roosevelt administration after national polls had revealed that almost three-fourths of the American people favored the legislation.[14] Although the sponsor of the amendment, Representative Louis Ludlow (D.-Ind.), argued that referendum was "the only way a question of such transcendent importance should be decided," the opportunity for the public to vote directly on issues of war and peace did not become widely available in this country for another thirty years.

At the height of the Vietnam controversy, several American communities held referenda on the war to permit residents to express their sentiments about this issue. As an unusual and specific context for the assessment of public attitudes toward war, the votes offered an important means of identifying segments of the population that might support or oppose military action abroad.

Although numerous studies have examined referenda on such diverse topics as fluoridation,[15] school bonds,[16] metropolitan government,[17] and civil rights controversies,[18] researchers usually have been compelled to investigate voting behavior on foreign policy issues in candidate elections rather than directly in referenda. One study of referendum voting derived from Hofstadter's distinction between the political ethos of "Yankee" and "immigrant" voters, however, alleged that upper class citizens were more likely to favor "public-regarding" proposals or measures justified by "the public interest" than lower-class residents.[19] Subsequent research has suggested that this phenomenon might be more closely related to socioeconomic than to ethnic characteristics[20] and that the concept of "public-regardingness" may have other limitations.[21] Yet, Hofstadter's original description of the upper- and middle-class ethos as denoting a belief that public policy should be developed "in accordance with general principles and abstract laws apart from and superior to personal needs" while the lower-class ethos "took for granted that the political life of the individual would arise out of family needs"[22] seemed to contain some useful implications for the study of foreign policy attitudes. The referenda concerning the Vietnam war thus provided a rare opportunity to explore this speculation by investigating voting behavior on foreign policy issues.

I. COMMUNITY REFERENDA ON THE VIETNAM WAR

Between 1966 and 1968, at least seven cities in the United States—Dearborn, Michigan; San Francisco, California; Cambridge, Massachusetts; Madison, Wisconsin; Mill Valley, California; Lincoln, Massachusetts; and Beverly Hills, California—held local referenda on the Vietnam war.[23] Election returns and comparable census tract data were obtained within each of the communities, except Lincoln and Mill Valley, to provide a basis for the examination of referendum voting on the war in Vietnam.[24]

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[14] Richard Dean Burns and W. Addams Dixon, "Foreign Policy and the 'Democratic Myth': The Debate on the Ludlow Amendment," *Mid-America,* 47 (October 1965), 288–306.

[15] Thomas F. A. Plaut, "Analysis of Voting Behavior on a Fluoridation Referendum," *Public Opinion Quarterly,* 23 (Summer 1959), 213–233; Harlan Hahn, "Voting Behavior on Fluoridation Referendums: A Re-evaluation," *The Journal of the American Dental Association,* 71 (November 1965), 1138–1144.

[16] Richard F. Carter and William G. Savard, *Influence of Voter Turnout on School Bond and Tax Elections,* U. S. Department of Health, Education and Welfare, Office of Education, Cooperative Research Monograph No. 5 (Washington: Government Printing Office, 1961).

[17] James A. Norton, "Referenda Voting in a Metropolitan Area," *Western Political Quarterly,* 16 (March 1963), 195–212; Richard A. Watson and John H. Romani, "Metropolitan Government for Metropolitan Cleveland: An Analysis of the Voting Record," *Midwest Journal of Political Science,* 5 (November 1961), 365–390.

[18] Norman I. Lusting, "The Relationship Between Demographic Characteristics and Pro-Integration Vote of White Precincts in a Metropolitan Southern County," *Social Forces,* 40 (March 1962), 205–208; James W. Vander Zanden, "Voting on Segregationist Referenda," *Public Opinion Quarterly,* 25 (Spring 1961), 92–105; Harlan Hahn, "Northern Referenda on Fair Housing: The Response of White Voters," *Western Political Quarterly,* 21 (September 1968), 483–495; Raymond E. Wolfinger and Fred I. Greenstein, "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," this REVIEW, 62 (September 1968), 753–769.

[19] James Q. Wilson, and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," this REVIEW, 58 (December 1964), 876–887.

[20] Harlan Hahn, "Ethos and Social Class: Referenda in Canadian Cities," *Polity,* 2 (Spring 1970), 295–315.

[21] Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," this REVIEW, 60 (June 1966), 306–326.

[22] Richard Hofstadter, *The Age of Reform* (New York: Alfred A. Knopf, 1955), p. 9.

[23] For a description of the Cambridge referendum, see Victoria Bonnell and Chester Hartman, "Cambridge Votes on the Vietnam War," *Dissent,* 15 (March-April 1968), 103–106. For an account of

By focusing on the comparative study of referendum votes, this research will attempt both to investigate recurrent voting patterns and to assess the responses of separate local electorates to the Vietnam issue.[25] Although individual behavior cannot, of course, be precisely inferred from ecological correlations,[26] the analysis of voting data at least permits the identification of the characteristics of segments of communities that were related to voter preferences concerning American participation in the Vietnam war.[27] In this study, survey data also will be used to supplement and verify the correlates of opposition to the war disclosed by referendum voting behavior.

In addition to the unusual opportunities that the referenda offered for direct public participation in foreign policy decisions, the voting may have been partially shielded from the conventional influence of national loyalties that often shape public attitudes on international issues.[28] As a logical extension of democratic procedures, referenda probably have evoked a relatively useful and accurate expression of voter preferences regarding the war in Vietnam.[29]

Furthermore, the referenda afforded a rare occasion to examine the distribution of sentiments on the Vietnam war within specific local areas that normally cannot be adequately sampled in national surveys. Since the characteristics of cities that held referenda on Vietnam policy differed appreciably, this study also has sought to explore the effects of local traditions and social structure on the correlates of public attitudes regarding a major political controversy.

Although the referendum results recorded slightly greater support for the withdrawal of American troops from Vietnam than concurrent national surveys,[30] the vote against the war in five cities between 1966 and early 1968 differed by no more than four percent. At the mid-term elections of 1966, 41 percent of the votes cast in Dearborn, Michigan endorsed a ballot proposition that was similar to the question used by university-sponsored surveys in asking, "Are you in favor of an immediate cease fire and withdrawal of United States troops from Vietnam so that the Vietnamese people can settle their own problems?" In municipal elections one year later, 39 percent of the voters in Cambridge favored "the prompt return home of American soldiers from Vietnam" and approximately 40 percent approved a similar proposal

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the first Dearborn referendum, see Harlan Hahn and Albert Sugarman, "A Referendum on Vietnam," *War/Peace Report,* 11 (May 1967), 14–15.

[24] Census tract data were not available for Lincoln or Mill Valley. Data for the other cities in this study were prepared by superimposing precinct boundaries on tract maps and reaggregating precinct voting statistics so that election and census districts were comparable. For a further description of this procedure, see Hahn, "Ethos and Social Class," 299. The author currently is conducting an extension study supported by U. S. Public Health Service grant No. DH 00151 of referendum voting behavior in approximately 60 U. S. cities employing social and economic variables obtained from city block statistics rather than from tract data.

[25] Austin Ranney, "The Utility and Limitations of Aggregate Data in the Study of Electoral Behavior," in Austin Ranney (ed.), *Essays on the Behavioral Study of Politics* (Urbana: University of Illinois Press, 1962), pp. 99–100.

[26] W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review,* 15 (1950), 351–357; Herbert Menzel, "Comment on Robinson's 'Ecological Correlations and the Behavior of Individuals,'" *American Sociological Review,* 15 (1950), 674–675.

[27] This study will attempt to compare not only voting patterns between cities but also the sentiments of relatively high or low status segments of the electorate both within the same community and in different communities. For a statement about the utility of focusing upon the study of social areas, or census tracts, rather than individuals, see Robert C. Tryon, "Identification of Social Areas by Cluster Analysis," *University of California Publications in Psychology,* 8 (August 1955), 4.

[28] Vander Zaden has noted that "since voting does take place secretly and privately—not publicly, where other social concerns, pressures, or considerations might intervene—it may be quite indicative of private sentiments." Vander Zaden, "Voting on Segregationist Referenda," 103.

[29] Perhaps referenda also have given citizens an increased opportunity to project their personal sentiments about the war in their votes rather than asking them to respond to the specific alternatives posed by an interviewer, which usually have focused on the international rather than the personal implications of foreign policy proposals.

[30] Skolnick, *The Politics of Protest,* p. 44; Robinson and Jacobson, "American Public Opinion about Vietnam," 71–72.

in San Francisco. Similarly, opposition to "the continued United States prosecution" of the Vietnam war in February, 1968 in Lincoln, Massachusetts was recorded at 39 percent. On April 2, in the Wisconsin primary 43 percent of the voters in Madison approved a proposal for "an immediate cease fire and withdrawal of United States troops from Vietnam."

One week later, and ten days after President Johnson's dramatic decision to limit the bombing of North Vietnam, the trend of the public opinion seemed to shift slightly as two California communities—Beverly Hills and Mill Valley—passed anti-war resolutions by identical margins of 56 percent. A virtual replication of the Dearborn referendum in November, 1968 also resulted in the approval of a proposal for unilateral withdrawal by 57 percent of the voters in that community. While the votes held after the Wisconsin primary reflected somewhat greater opposition to the war than the elections held before then, the results of separate referenda during both periods seemed to be essentially similar.

II. SOCIOECONOMIC CHARACTERISTICS AND THE VOTE ON THE VIETNAM WAR

The referendum voting returns permitted a comparative investigation of public sources of support or opposition concerning American participation in the Vietnam war. The Pearsonian product-moment coefficients of correlation between conventional measures of socioeconomic status such as income, education, and occupation and the percentage of the vote against American participation in the Vietnam war in six referenda are presented in Table 1.[31]

Perhaps most striking relation shown in Table 1 is the strong *inverse* association between opposition to the Vietnam war and increasing socioeconomic status. Disapproval of the war appeared to be related to working-class rather than to high-status characteristics. In most communities, as the proportion of voters possessing relatively low-status attributes grew, the vote against the Vietnam war also tended to mount.

The voting, however, did reveal some of the unique features of each locality. In Dearborn and San Francisco, for example, the percentage of persons earning an annual income of less than $7,000 was directly related to the vote against the war, while the proportion earning wages higher than that figure was negatively associated with anti-war voting; but, in the exceedingly affluent environs of Beverly Hills, the correlations did not reveal an inverse association between income and opposition to the war until the salary level of $15,000 was reached. On the other hand, in Madison, voting for the withdrawal of American troops was directly related only to the percentage of people receiving wages of less than $5,000 annually; and it was inversely related to the proportions earning larger sums.[32] The association between educational attainments and the vote against the Vietnam war in Beverly Hills was inverse only for the variable measuring college attendance; but in Dearborn this relationship was inverse for the percentage of people that completed high school, and in San Francisco it was negatively related to all groups that had gone beyond the eighth grade. Comparable patterns were evident in the associations between occupational characteristics and the vote in Dearborn, San Francisco, and Beverly Hills. *Relative* to the aggregate socioeconomic level of most communities, therefore, the data indicated that attributes signifying lower social status were related to increasing opposition to the war in Vietnam.

Perhaps the clearest exception to this generalization, however, was the referendum vote in Cambridge. In that municipality, opposition to the war was directly related to the proportion of persons earning incomes of $10,000 or more annually, the college-educated, and professional or managerial employees; but it was inversely associated with the percentage of people exhibiting lower educational, salary, or occupational attainments. In part, this pattern may have been produced by unique features of the Cambridge referendum campaign which, after a bitter struggle with a Progressive Labor Party group that circulated a counter-petition condemning the war for exploiting American workers, was conducted and led by what two participant-observers called "a middle-class, university-oriented" organization that virtually ignored working-class neighborhoods. Despite this neglect, the observers concluded, "Support for the referendum was lower than anticipated in the predominantly upper-income university sections but substantially

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[31] Although the variables or categories of income, education, and occupation are both inter-related and ipsative—that is, the proportion of persons at one level will affect the percentage at another level, they are not mirror images of each other. Consequently, all of the classifications have been reported both to indicate the patterns or the thresholds reflected by the correlations and to provide a more complete picture of the associations between socioeconomic status and the vote on Vietnam policy than would be possible through the use of summary measures.

[32] The mean vote against the Vietnam war in Madison ranged from 59 percent in tracts with a median income below $4,000 to 37 percent in tracts where the median income was $8,000 or more.

TABLE 1. COEFFICIENTS OF CORRELATION BETWEEN SOCIOECONOMIC CHARACTERISTICS AND THE VOTE AGAINST U.S. PARTICIPATION IN THE VIETNAM WAR BY CENSUS TRACT IN FIVE CITIES, 1966–1968

	Dearborn (1966) (N=18)	Dearborn (1968) (N=18)	Beverly Hills (1968) (N=5)	San Francisco (1967) (N=122)	Madison (1968) (N=27)	Cambridge (1967) (N=30)
Income:						
Percent of population earning less than $5,000 annually	+.83***	+.82***	+.70	+.39***	+.57***	−.27
Percent of population earning $5,000–$6,999 annually	+.66***	+.78***	+.74	+.19*	−.06	−.59***
Percent of population earning $10,000–$14,999 annually	−.81***	−.85***	+.53	−.44***	−.41*	+.37*
Percent of population earning more than $15,000 annually	−.50*	−.64**	−.67	−.30***	−.19	+.54***
Education:						
Percent of population with 8 years of schooling or less	+.79***	+.81***	+.40	+.24**	+.00	−.71***
Percent of population with 9–11 years of schooling	+.53*	+.68***	+.80	−.03	−.13	−.74***
Percent of population with high school degrees	−.45*	−.34	+.53	−.38***	−.30	−.55***
Percent of population with college education	−.65**	−.78***	−.75	−.18*	−.24	+.81***
Occupation:						
Percent of population employed in professional or managerial jobs	−.66***	−.80***	−.63	−.30***	+.00	+.76***
Percent of population employed in clerical or sales jobs	−.66***	−.68***	+.73	−.22**	−.14	−.14
Percent of population employed as craftsmen or foremen	+.11	+.28	+.43	−.11	−.38*	−.44**
Percent of population employed as operatives or laborers	+.71***	+.81***	+.23	+.16*	−.13	−.70***
Percent of population unemployed	+.51*	+.40*	+.75	+.39***	+.44**	−.44**

* significant at the .05 level
** significant at the .01 level
*** significant at the .001 level

higher than anticipated in the working-class districts."[33] In addition, voting patterns on other political controversies and referendum issues in this city long have been influenced by a traditional "cleavage between the 'Yankee' residents of 'Old Cambridge' and the Irish and Italians who live primarily in 'East Cambridge.'"[34] The peculiar circumstances surrounding the referendum campaign in Cambridge as well as local social animosities, therefore, probably contributed to the marked differences between the socioeconomic correlates of the vote there and the patterns uncovered in other cities.

Although the findings from the other major university community of Madison also were somewhat unclear, the data revealed few other contrasts between voter preferences in large cities such as San Francisco and suburban communities such as Dearborn and Beverly Hills or between the periods in which the referenda were held. The social and economic sources of opposition to the Vietnam conflict in 1966 or 1967, when criticism of the war was limited, did not seem to differ appreciably from those reflected in the referenda of 1968, by which time hostility to the war had broadened and intensified. In fact, the nearly identical referenda held in Dearborn in 1966 and 1968, respectively, seemed to indicate no major changes in the distribution of attitudes or voting patterns on the Vietnam issue, except that the associations between lower-class attributes and sentiments favoring an end to the Vietnam war seemed to increase, while the corresponding relationships between high-status indicators and support for the war also were enhanced.

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[33] Bonnell and Hartman, "Cambridge Votes on the Vietnam War," 105–106.

[34] Hahn, "Voting Behavior on Fluoridation Referendums," 1142.

The socioeconomic discrepancies between supporters and opponents of the war reflected in referendum voting also were confirmed by survey data. The Gallup Poll found, for example, that high income and well educated respondents were most likely to describe their attitudes toward the Vietnam war as "hawkish," while a large proportion of persons with grade school educations chose the designation "doves."[35] Manual workers also expressed stronger support for both the immediate and the gradual withdrawal of American troops from Vietnam than persons in business or professional occupations.[36] Moreover, investigations of a 1966 survey of 406 adults in Detroit using the concept of "social position" developed by Galtung[37] revealed that *both* support for a de-escalation of the Vietnam war and the desire to participate in a referendum on Vietnam policy were inversely related to increasing social positions.[38]

While the findings seemed to contradict much popular speculation about the alleged resistance of working-class voters to the anti-war movement, they did not appear to be inconsistent with some appraisals of the social and economic effects of war. In general, young men from high income families are less likely to be drafted than low status males.[39] Other statistics have indicated that a disproportionate number of youths from low socioeconomic origins and minority groups are assigned to Vietnam, and those soldiers may have suffered increased casualties.[40] In many lower-class homes, the actual or threatened loss of a son perhaps has implied economic hardship as well as personal tragedy; and more working-class than high-status families apparently have been exposed to this risk. The Vietnam war probably has had a particular impact on lower-class voters that affected the preferences on the issue they expressed at the ballot box.

III. HOUSEHOLD CHARACTERISTICS AND THE VOTE ON THE VIETNAM WAR

Since family interests, especially in homes that contain draft-age sons, may have had an important effect on public attitudes toward the Vietnam war, variables that reflected the age, ethnic, social, or economic attributes of families were related to referendum voting behavior. The coefficients of correlation between selected household characteristics and the vote in opposition to the Vietnam war are contained in Table 2.

The referenda tended to confirm the findings of earlier surveys that a relatively strong "predictor of attitudes toward Vietnam policy was race."[41] Although the proportions of black residents in Dearborn and Beverly Hills were so small as to make the correlations virtually meaningless,[42] race was associated with opposition to the war in Madison and Cambridge, where black voters comprised a small segment of the community, and in San Francisco, which contained a sizeable black population.

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[35] *Gallup Opinion Index,* Report No. 40 (October 1968), 25.

[36] *Gallup Opinion Index,* Report No. 49 (July 1969), 10–11; "Americans on the War: Divided, Glum, Unwilling to Wait," *Time,* 94 (October 31, 1969), 13–15. In May, 1970, blue collar workers lacking a high school education apparently expressed strong opposition to President Nixon's action in Cambodia. "A Newsweek Poll: Mr. Nixon Holds Up," *Newsweek,* 75 (May 25, 1970), 30.

[37] Johan Galtung, "Foreign Policy Opinion as a Function of Social Position," *Journal of Peace Research,* 1 (1964), 206–231; Johan Galtung, "Social Position, Party Identification and Foreign Policy Orientation: A Norwegian Case Study," in Rosenau (ed.), *Domestic Sources of Foreign Policy,* 161–193.

[38] Harlan Hahn, "Dove Sentiments Among Blue-Collar Workers," *Dissent,* 17 (May-June, 1970), 202–205.

[39] James W. Davis, Jr. and Kenneth M. Dolbeare, "Selective Service and Military Manpower: Induction and Deferment Policies in the 1960's," in Austin Ranney (ed.), *Political Science and Public Policy* (Chicago: Markham, 1968), pp. 83–121.

[40] "Negroes and Military Service—Latest Facts," *U. S. News and World Report* (August 15, 1966); see also Ralph Guzman, "Mexican-American Casualties in Vietnam," unpublished paper, Merrill College, University of California, Santa Cruz, 1969.

[41] Verba, *et. al.,* "Public Opinion and the War in Vietnam," 325.

[42] In Dearborn and Beverly Hills, however, the percentage of foreign-born residents was closely-related to the vote against American participation in the war.

TABLE 2. COEFFICIENTS OF CORRELATION BETWEEN SELECTED HOUSEHOLD CHARACTERISTICS AND THE VOTE AGAINST U.S. PARTICIPATION IN THE VIETNAM WAR BY CENSUS TRACT IN FIVE CITIES, 1966–1968

	Dearborn (1966) (N = 18)	Dearborn (1968) (N = 18)	Beverly Hills (1968) (N = 5)	San Francisco (1967) (N = 122)	Madison (1968) (N = 27)	Cambridge (1967) (N = 30)
Children in Households:						
Percent of households containing children under 18 years of age	−.60**	−.56**	−.33	+.22**	−.77***	−.63***
Percent of males aged 10–19 years	−.80***	−.82***	−.62	+.05	+.48**	−.23
Percent of males aged 15–24 years	+.23	+.23	−.84*	−.09	−.54**	−.37*
Age of Males:						
Percent of males aged 20–34 years	+.76***	+.83***	+.62	+.37***	+.63***	+.53***
Percent of males aged 35–44 years	−.75***	−.73***	+.01	+.20*	−.68***	−.46**
Percent of males aged 45–59 years	−.26	−.35	−.82*	−.43***	−.23	−.16
Percent of males over 60 years of age	+.81***	+.72***	+.25	−.35***	+.11	+.13
Ethnicity:						
Percent of population nonwhite	+.25	+.11	−.57	+.65***	+.21	+.15
Percent of population foreign born	+.81***	+.79***	+.98**	−.16*	+.34*	−.67***
Home Ownership or Value:						
Percent of owner occupied dwelling units	−.80***	−.67***	−.44	−.30***	−.70***	−.32*
Percent of homes valued at less than $10,000	+.67***	+.74***	+.14	+.19*	+.58***	−.58***
Percent of homes valued at $10,000–$19,999	−.06	+.07	+.58	+.18*	+.05	−.26
Percent of homes valued at $20,000–$25,000	−.51*	−.64**	+.96**	−.05	−.27	+.51**
Percent of homes valued at more than $25,000	−.41	−.54**	−.96**	−.19*	−.14	+.66***
Average Monthly Rent:						
Percent of residents renting for less than $60	+.68***	+.58**	+.34	+.36***	+.64***	−.37*
Percent of residents renting for $60–$99	+.64**	+.70***	+.61	+.01	+.29	−.36*
Percent of residents renting for $100–$149	−.80***	−.77***	+.52	−.37***	−.33*	+.64***
Percent of residents renting for more than $150	−.56**	−.64**	−.51	−.26**	−.44*	+.50**

* significant at the .05 level
** significant at the .01 level
*** significant at the .001 level

In addition, the data on household characteristics seemed to underscore the influence of socioeconomic attributes on referendum voting. In all cities, for example, renters apparently expressed greater opposition to the Vietnam war than homeowners. Furthermore, lower house values and rents were directly associated with the vote against the war in four of the cities; but more expensive homes and rents were inversely related to the protest vote. Generally, differences in the associations with the vote began to appear as house values approached $10,000 and as average monthly rents neared $100, except in the high-status community of Beverly Hills, where the signs of the correlations changed markedly at the levels of more than $25,000 for home values and of $150 or more for mean monthly rent. Although the relationships between the vote on the Vietnam war and both the value of dwelling units and average monthly rent in Cambridge again were inconsistent with the associations found elsewhere, in the remaining cities opposition to the war was related to such working-class attributes as an annual income below $5,000, less than 8 years of schooling, unskilled occupations, unemployment, low home ownership, house values below $10,000, and monthly rents of $100 or less.

Family characteristics, however, seemed to be more closely related to opposition to the Vietnam war than might have been expected. In fact, the proportion of housholds containing children under 18 years of age was inversely related to the vote against the war in all cities except San Francisco. Moreover, the percentage of young men between 10 and 19 years old, or approaching draft age, was negatively associated with disapproval of the war in the Dearborn, Beverly Hills, and Cambridge referenda. In Beverly Hills, even the proportion of men in a prime category of draft eligibility, aged 15–24 years, was inversely related to the vote in opposition to the war; and it was significantly correlated with voting behavior only in Madison and Beverly Hills. Variables reflecting the age of children, therefore, did not seem to yield particularly strong or consistent associations with positions on the Vietnam war.

The inverse associations between the vote against the war and the proportion of young men as well as middle-aged parents seemed to be inconsistent with the apparent interests of those segments of the population; yet, many citizens have professed a willingness during wartime to sublimate their personal concerns for the sake of national loyalty. Perhaps opposition to the Vietnam war has been inspired less by the draft status of young males than by the social status of voters who may hold different perceptions of "the public interest" or national obligations. While the probability of induction into military service has varied by social class, disapproval of American participation in the Vietnam war apparently was more closely related to socioeconomic attributes than to the potential threat of the draft.

IV. CORRELATES OF THE VOTE ON THE VIETNAM WAR

To illustrate the major associations between ecological characteristics and voting behavior in the Vietnam referenda, 12 variables that were significantly related to the vote were chosen from an initial analysis of 198 attributes. The coefficients of correlation between selected socioeconomic and household characteristics and the vote against American participation in the Vietnam war are presented in Table 3.

The combined effects of the twelve variables as determined by multiple regression analysis seemed to be closely associated with referendum voting on Vietnam policy. The multiple correlation coefficients (R) ranged from .80 in San Francisco to .99 in Dearborn and Beverly Hills, indicating that the variables accounted for a sizeable amount of the variation (R^2) in the votes. Demographic characteristics, therefore, appeared to be strongly related to the vote on the Vietnam war.

The summary measures also revealed that median family income was inversely associated with opposition to the war, even in university centers such as Madison and Cambridge. In fact, some evidence indicated that the association between socioeconomic status and voting on the Vietnam issue in Cambridge and Madison may have reflected the unusual distribution of educational characteristics in those campus communities. When median education was employed as a control variable, the partial correlations between median income and the vote against the war increased to $-.58$ in Cambridge and to $-.87$ in Madison. The relationship between median family income and opposition to the war, therefore, seemed to broaden somewhat the scope of the finding that socioeconomic status was inversely related to the disapproval of American military activities in Vietnam.

Variables denoting the proportions of households that contained draft-age sons did not seem to be uniformly associated with opposition to the war, but the percentages of men aged 36 to 60 years were inversely related to the vote against the Vietnam war in all cities. In fact, the only variables that yielded consistent or unidirectional correlations in all communities were median family income, the percentage of owner

TABLE 3. COEFFICIENTS OF CORRELATION BETWEEN SELECTED SOCIOECONOMIC OR HOUSEHOLD CHARACTERISTICS AND THE VOTE AGAINST U.S. PARTICIPATION IN THE VIETNAM WAR BY CENSUS TRACT IN FIVE CITIES, 1966–1968

	Dearborn (1966) (N=18)	Dearborn (1968) (N=18)	Beverly Hills (1968) (N=5)	San Francisco (1967) (N=122)	Madison (1968) (N=27)	Cambridge (1967) (N=30)
Median family income	−.79***	−.85***	−.64	−.41***	−.66***	−.33*
Median education	−.75***	−.78***	−.72	−.14	+.22	+.74***
Percent of population employed in white collar jobs	−.70***	−.81***	−.27	−.30***	−.03	+.72***
Percent of population unemployed	+.51*	+.40*	+.75	+.39***	+.44**	−.44**
Percent of owner occupied dwelling units	−.80***	−.67***	−.44	−.30***	−.70***	−.32*
Median value of dwelling unit	−.60**	−.74***	−.62	−.14	−.39*	+.72***
Median monthly rent	−.84***	−.85***	−.66	−.35***	−.39*	+.60***
Percent of population nonwhite	+.25	+.11	−.57	+.65***	+.21	+.15
Percent of population foreign born	+.81***	+.79***	+.98**	−.16	+.34*	−.67***
Percent of households containing children under 18 years of age	−.60**	−.56**	−.33	+.22**	−.77***	−.63***
Percent of males aged 15–24 years	+.23	+.23	−.84*	+.12	+.76***	+.21
Percent of males aged 35–60 years	−.71***	−.76***	−.67	−.09	−.54**	−.37*
Multiple R	.99	.99	.99	.80	.98	.96
Multiple R^2	.99	.99	.99	.65	.96	.93

* significant at the .05 level
** significant at the .01 level
*** significant at the .001 level

occupied dwelling units, and the proportion of men between 35 and 60 years of age. Each of those characteristics was inversely related to opposition to the war in all six referenda.

This finding raised the disquieting possibility that the association between age and the vote on the Vietnam issue, instead of indicating the concern of fathers for the safety of their sons, actually may have signified generational conflict or the hostility of middle-aged voters toward youthful anti-war protestors. Perhaps many middle-aged parents endorsed existing Vietnam policy in the referenda as a reflection of their faith and confidence in the country or as an expression of their disapproval of student demonstrations to end American involvement in the Vietnam war. Contrasting perceptions of the war and the national interest may have represented generational as well socioeconomic differences. Since popular speculation has emphasized the disagreements between young people and the older generation over the Vietnam issue, this interpretation seemed to require further examination.

V. AGE AND SOCIOECONOMIC CHARACTERISTICS AND THE VOTE ON THE VIETNAM WAR

Since increasing social status and advancing age usually have been highly inter-related, median family income was used as a control variable in the computation of partial coefficients of correlation between several demographic characteristics and the vote against the Vietnam war in four cities.[43] The results of this analysis are presented in Table 4.

The partial correlations suggested that income

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[43] Due to limitations on degrees of freedom in some cities, only first-order partial correlations were used in this study. Missing data for some variables also necessitated the elimination of Beverly Hills from this analysis.

TABLE 4. PARTIAL COEFFICIENTS OF CORRELATION† BETWEEN SELECTED SOCIOECONOMIC OR HOUSEHOLD CHARACTERISTICS AND THE VOTE AGAINST U.S. PARTICIPATION IN THE VIETNAM WAR BY CENSUS TRACT IN FOUR CITIES, 1966–1968

	Dearborn (1966) (N=18)	Dearborn (1968) (N=18)	San Francisco (1967) (N=122)	Madison (1968) (N=27)	Cambridge (1967) (N=30)
Median education	+.16	+.02	−.00	+.56*	+.77***
Percent of population employed in white collar jobs	+.55*	+.10	−.08	+.19	+.76***
Percent of population unemployed	+.28	+.47	+.28**	−.12	−.47**
Percent of owner occupied dwelling units	−.79**	−.54*	+.21*	+.00	+.02
Median value of dwelling units	+.58*	+.24	−.14	+.20	+.79***
Median monthly rent	−.37	−.34	−.07	+.56*	+.74***
Percent of population nonwhite	+.07	−.40	+.62***	+.13	+.18
Percent of population foreign born	+.46	+.62*	−.00	−.42	−.75***
Percent of households containing children under 18 years of age	−.44	−.59*	+.38***	+.00	−.52**
Percent of males aged 15–24 years	−.37	−.22	+.35***	+.59*	+.16
Percent of males aged 35–60 years	+.21	+.14	−.09	−.34	−.35*

† controlling on median family income
* significant at the .05 level
** significant at the .01 level
*** significant at the .001 level

may have been a primary determinant of the vote in opposition to the Vietnam war. The associations between disapproval of the war and other socioeconomic indicators such as education, occupation, home ownership, house value, and monthly rent, with the effects of median income removed, were either reversed or substantially reduced. In fact, several socioeconomic characteristics displayed a strong direct rather than inverse association with the vote against the war after partialling on median income. This pattern appeared in most of the cities except San Francisco, although it was particularly evident in Cambridge. Race remained strongly related to the protest vote in San Francisco, but there were few other particularly striking or meaningful correlates of the vote independent of the influences of family income. None of the variables yielded correlations that were consistent in direction or sign after the intervening impact of median income had been excluded.

In addition, the associations between the vote and several age variables—including the percent of children under 18, the proportion of young males between 15 and 24, and the percentage of men between 35 and 60—were not strengthened, but they were generally reduced or reversed in the partial correlations controlling on median family income. The inverse association between disapproval of the war and the proportion of men between 35 and 60 years of age, for example, remained weak in San Francisco; it was diminished somewhat in Cambridge and Madison; and it became mildly positive in the two Dearborn referenda. On the other hand, the partial correlations between opposition to the war and median family income, controlling on the percentage of men between 35 and 60 years of age, were only lowered slightly; and they remained consistent in all communities, yielding the following inverse relationships: Dearborn (1966), −.57; Dearborn (1968), −.55; San Francisco, −.51; Cambridge, −.23; and Madison, −.62. The partial correlations, therefore, suggested that voter preferences in referenda on the Vietnam issue may have been less closely or consistently related to generational differences than to socioeconomic divisions within the community.

VI. SUMMARY AND DISCUSSION

Voting behavior in local referenda on the Vietnam war seemed to be strongly associated with demographic characteristics. In fact, a multiple regression analysis indicated that twelve basic personal or household attributes acting together accounted for a sizeable amount of variance in the votes. Yet, some interesting differences were discovered in the associations between specific variables and public support or opposition concerning American involvement in the Vietnam war.

Although voting patterns in municipal referenda on the Vietnam issue may have been affected by the social structure of the entire community as well as by local traditions and rivalries, the data generally indicated that opposition to the war was associated with working-class rather than middle- or upper-income attributes. In most of the referenda, the vote to end U. S. participation in the Vietnam war was directly related to relatively low income, education, occupational status, home ownership, house values, and monthly rent, while relatively high positions on those measures were associated with the endorsement of existing policies in Vietnam. In part, this finding may have reflected the special burdens that wars have imposed on lower-class voters.

In addition, opposition to U. S. military intervention in Vietnam was inversely related to median income in each of the cities. Partial correlations, employing median family income as a control variable, seemed to underscore the importance of socioeconomic influences on the vote in Vietnam referenda. The associations between protest voting and the proportion of middle-aged men as well as other age groups seldom remained consistent or significant when the impact of income was excluded, but partial correlations between income and opposition to the war were uniformly inverse and statistically significant in four referenda with the effects of middle age removed. Opposition to American participation in the Vietnam war, therefore, seemed to be reflected more by social and economic distinctions than by the disagreements that have divided young people and their elders.

Perhaps attitudes about war have been shaped by the simple fact that most members of the public, and especially working-class voters, seldom have been given an opportunity to exert a direct influence on foreign policy decisions. For very small sectors of the population, the chance to express personal convictions about the Vietnam war has been available through picketing and mass protests; but it has been difficult to determine whether or not the demonstrations accurately reflected popular sentiments about the issue. The relatively remote nature of foreign policy questions has seemed to greatly complicate the search for correlates of public opinion regarding war.

The national survey, which reported that social and economic characteristics were not related to attitudes toward the war, for example, also disclosed that the opinions about the Vietnam war held by informed and articulate respondents did not differ appreciably from the views expressed by persons who lacked those characteristics.[44] Subsequent analysis of a survey of Detroit adults, however, revealed that the greatest desire to vote in a referendum on Vietnam policy was centered among persons who displayed a low sense of political efficacy but who were highly interested in world problems; those respondents also expressed the strongest support for a de-escalation of the Vietnam war.[45]

Since feelings of political ineffectiveness have been most prevalent among relatively low-status voters,[46] perhaps a tendency to defer to the statements of supposedly expert national leaders and the lack of a strong sense of efficacy promoted the common perceptions of working class "hawkishness" during the Vietnam controversy. Increased opportunities for the public to vote directly on issues of war and peace, therefore, conceivably could have revealed a slightly altered distribution of popular sentiments about the Vietnam issue.

The inverse association between increasing socioeconomic status and opposition to the Vietnam war found in most communities seemed to suggest some important qualifications of existing speculation about public attitudes on political issues. The tendency of lower class voters to favor a conciliatory rather than a militaristic solution to the Vietnam conflict, for example, did not seem to be consistent with the notion of "working class authoritarianism."[47] One researcher even has suggested that the findings of recent studies of attitudes regarding war may indicate the presence of a characteristic "which, at least with respect to this kind of foreign policy concern, we might label as 'upper middle class authoritarianism.' "[48]

The results of this investigation, however, did not seem to contradict speculation about the concept of an upper-class "Yankee-Protestant"[49]

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[44] Verba, *et al.*, "Public Opinion and the War in Vietnam," 326–330.

[45] Harlan Hahn, "Political Efficacy and Foreign Policy Attitudes," *Social Problems*, 17 (Fall 1969), 271–279.

[46] See Lester W. Milbrath, *Political Participation* (Chicago: Rand, McNally, 1965), pp. 56–58.

[47] Seymour M. Lipset, "Democracy and Working-Class Authoritarianism," *American Sociological Review*, 24 (August 1959), 482–502.

[48] Hamilton, "A Research Note on the Mass Support for 'Tough' Military Initiatives," 442.

[49] Hofstadter, *The Age of Reform*, pp. 9–10.

or "public-regarding"[50] ethos that usually has implied support for public policies or referendum measures that are sanctioned by "the public interest" or by "the welfare of the community." As a collective action that often has been justified as necessary for the protection or preservation of the entire society, the endorsement of national participation in a major war seemed to be consistent with the political ethos of relatively high-status segments of the community. On the other hand, working-class residents probably have tended to adopt political positions on the basis of personal or family considerations. Among lower-class voters, who have been inclined to interpret political controversies in terms of simple or immediate needs and obligations, perhaps attitudes concerning war have been shaped less by the perceived interests of the entire country than by the potential loss of young lives.[51]

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[50] Wilson and Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," 876–887.

[51] Impressionistic, unstructured interviewing conducted after the first vote on the Vietnam war in Dearborn, Michigan, in fact, suggested a sharp distinction between the "ideological" reactions to the referendum issue that were elicited in relatively affluent sections of the community and the highly "personal" responses that emerged in working-class neighborhoods where the proposal to withdraw American troops from Vietnam received a clear majority. While many high-status voters evaluated the controversy as a necessary defense against the global threat of Communist expansion or as an unwarranted intervention in a civil war, most working-class residents responded to the war as a tangible and direct threat to the lives of American men. Since their interpretation of the war was confined to the death and injury that it implied, the perspectives of working-class voters left them with few alternatives except to oppose the war. See Hahn and Sugarman, "A Referendum on Vietnam," 14–15.

POLITICAL TRUST AND RACIAL IDEOLOGY*

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I. INTRODUCTION

No government yet established has had the loyalty and trust of all its citizens. Regardless of the popularity of its leaders or how careful they are in soliciting opinions and encouraging participation in the process of policy-making, there are always those who see inequalities and injustices in the society and harbor suspicions of the government's motives and intentions. Resentment and distrust are elements of disaffection and the first step toward resistance. Therefore, even the most dictatorial governments have usually striven to increase their credibility and popularity. For democratic governments, however, the problem of combating distrust and encouraging voluntary acceptance of its institutions and decisions is a paramount concern. One of democratic theory's distinctive characteristics is its strong emphasis on voluntary consent, both as a basis of political obligation and as a central attribute of citizenship. The concern expressed by democratic thinkers about the elements of due process and the protection of opportunities for widespread participation is directed toward the creation of citizens who voluntarily accept the society's goals; "the demand for consent is the demand that the government must be more than self-appointed and must, in some significant way, be the chosen instrument through which the body politic and community acts. . . ."[1]

Democracy's guiding ideal is the substitution of mutual understanding and agreement for coerciveness and arbitrary authority in all phases of social and political life. The existence of distrustful citizens who are convinced that the government serves the interests of a few rather than the interests of all is a barrier to the realization of the democratic ideal. In Sabine's words: "full understanding cannot be reached except on the basis of mutual respect and with a mutual acknowledgment of good faith and the acceptance of the principle that the purpose of understanding is to protect all valid interests."[2] Leaders in a representative democracy cannot be successful until they have gained the trust of the citizens; this is even more important in American society where racial and ethnic minorities are actively searching for a new, more dignified role as political equals.

Besides its important normative implications, the level of trust in government also is an important determinant of political change. The rise and fall in the number of distrustful citizens over time is a sensitive barometer of social conflicts and tensions. When any sizable group becomes distrustful and begins to make demands, the government is prompted to reallocate its resources or change its institutions to accommodate these new pressures. If the political system is flexible and adaptive enough, needed adjustments can be made without any consequent outbreak of violence, but if distrustful groups are denied access to decision-makers, or if institutions are too rigid to change, destructive conflict and a breakdown in the social order are possible. Under the right political conditions, distrustful groups, which exist in all societies, may produce the kind of creative tensions needed to prompt social change, but under other conditions, these same tensions either may lead to violent disruption or indiscriminate and cruel repression. A society's leaders may either strive to meet the demands of the distrustful group, or instead, they may endeavor to isolate and attack the group, making it a scapegoat for the resentment and hostility of the majority. Rising distrust is often a stimulant to social change, but its consequences depend on the response it provokes from leaders and other elements of the society.

The level of trust in government strongly influences the kind of policies and strategies available to political leaders. As Gamson has argued, when the level of trust is high, "the authorities are able to make new commitments on the basis of it and, if successful, increase such support even more. When it is low and declining, authorities may find it difficult to meet existing com-

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* The principal grant which supported this study came from the National Institute of Mental Health. Additional support also came from the Horace H. Rackham Faculty Research Fund and the Institute of Public Policy Studies, The University of Michigan. Thanks are due to Steven L. Coombs and William A. Gamson who read and criticized an earlier version of this paper and to James D. Chesney and Douglas B. Neal who assisted in the data analysis.

[1] Joseph Tussman, *Obligation and the Body Politic* (New York: Oxford University Press, 1960), p. 23.

[2] George Sabine, "The Two Democratic Traditions," *The Philosophical Review*, 61 (1952), p. 471.

mitments and to govern effectively."[3] Levels of trust and allegiance differ greatly among countries and these differences determine both the number of options open to the government and the relative danger of political fragmentation.[4] The distribution of trust among different social groups in the society also may have an important effect on the relative success of different governmental policies, and a sensitive awareness of its importance should allow a political leader to adopt more successful strategies of persuasion.

Widespread trust in government is recognized by students of both normative and empirical questions as the foundation for democratic order. This paper presents a comparative analysis of political trust in the black and white communities of Detroit, Michigan—a city which has a history of racial conflict and experienced major civil disturbances in 1943 and 1967. We discuss: (1) the concept of political trust; (2) the levels of trust in both racial communities; (3) the principal social and political sources of trustful and distrustful attitudes; (4) the contrast between the correlates and nature of political distrust in the black and white communities; (5) the behavioral consequences of distrust; and (6) the racial ideologies linked to political trust in both communities.

II. A REVIEW OF THE LITERATURE

Given the widely acknowledged importance of political trust in maintaining political stability or promoting change[5] it is surprising that empirical research on the origins and consequences of trust is so scarce. In addition, the small body of literature which does exist raises more questions than it answers. Studies contradict each other as conceptual and measurement problems abound. Sometimes political trust is clearly related to social status[6] and sometimes not.[7] Often it is correlated with feelings of political efficacy,[8] but not always.[9] In most instances it is strongly related to measures of trust in other people,[10] but again, not always.[11]

At least some of this confusion is due to the fact that the reported research has taken place in different settings. We know that levels of trust vary according to what Litt has called the "political milieu" in which distrust "may be acquired as a *community norm,* a part of the political acculturation process in the city's daily routine."[12] Not only can the political milieu influence the level of trust, but the relationships between variables are not always the same in each setting. For example, in Litt's comparative study of a middle class neighborhood in Boston and a comparable area in adjoining Brookline there is no relationship between feelings of political trust and political efficacy in Boston and yet there is a strong relationship in Brookline. The explanation offered is that in Boston, a city noted for blatant corruption in its political life, "community-wide suspicions of 'base practices' may go hand in hand with a belief that the professional practitioner of politics will still turn an attentive ear to the plaints of his constituents,"[13] while in Brookline, a community with a history of clean government, those who are distrustful are fully convinced that political leaders will not be responsive to their requests. In Brookline the citizens' distrust can be traced to personality variables, while in Boston the political milieu is the dominant factor.[14]

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[3] William A. Gamson, *Power and Discontent* (Homewood, Ill.: Dorsey, 1968), pp. 45–46.

[4] Gabriel A. Almond and Sidney Verba, *The Civic Culture: Political Attitudes and Democracy in Five Nations* (Princeton: Princeton University Press, 1963), p. 490.

[5] Gamson's work, *op. cit.,* builds on the concerns of Parsons and Easton. See, especially, Talcott Parsons, "Some Reflections on the Place of Force in Social Process" in Harry Eckstein (ed.), *Internal War* (New York: Free Press, 1964), pp. 33–70, and David Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965).

[6] Robert E. Agger, Marshall N. Goldstein and Stanley A. Pearl, "Political Cynicism: Measurement and Meaning," *The Journal of Politics,* 23 (1961), 477–506 and Herbert McClosky, "Consensus and Ideology in American Politics," this REVIEW (1964), 361–383.

[7] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment," in Harlan Cleveland and Harold D. Lasswell (eds.), *Ethics and Bigness: Scientific, Academic, Religious, Political and Military* (New York: Harper, 1962), pp. 61–73; and Joel D. Aberbach, *Alienation and Race* (unpublished Ph.D. Dissertation, Yale University, 1967), especially pp. 102–126 and 206–208.

[8] Stokes, *op. cit.,* p. 68; Agger, *op. cit.,* p. 494; and Edgar Litt, "Political Cynicism and Political Futility," *The Journal of Politics,* 23 (1963), p. 321, Table 5.

[9] Litt, *op. cit.,* p. 320, Table 2.

[10] See Morris Rosenberg, "Misanthropy and Political Ideology," *American Sociological Review,* 21 (1956), 690–695.

[11] Litt, *op. cit.,* p. 320, Table 1.

[12] *Ibid.,* p. 319.

[13] *Ibid.,* p. 320.

[14] *Ibid.,* pp. 317. Litt finds that the "degree of

A study of distrust at the federal level raised similar issues. Stokes, using data from the Survey Research Center's 1958 national election study, found a correlation between feelings of political efficacy and political trust. He hypothesized that under certain conditions one could find subjective powerlessness linked with a positive attitude toward government, but "in the context of democratic values, feelings of powerlessness toward public authority tend to create feelings of hostility toward the authority."[15] While Stokes reported a relationship between political trust and efficacy similar to that found among Litt's Brookline residents, his national sample resembled Litt's Boston respondents in that social status variables were at best weakly correlated with trust.

The setting of the research apparently is important in determining both the level of distrust and the relationship between it and other variables. If we are to explain successfully the origins and consequences of distrust, therefore, we must systematically introduce into our measures factors associated with the political settings of the population. To do so we must deal with conceptual problems which are intimately tied to problems of measurement. The concept of political distrust is defined by Stokes as a "basic evaluative orientation toward the . . . government."[16] However, items in many of the scales designed to measure political distrust often involve simple clichés about the quality of politics and politicians with little or no indication as to the governments or figures involved. McClosky, who employs such items in his measure, is quite concerned about their validity and therefore about the interpretation of his results. "It is," he says, "impossible in the present context to determine the extent to which scores contained in these tables signify genuine frustration and political disillusionment and the extent to which they represent familiar and largely ritualistic responses."[17] This is not to deny that some element of disaffection may be tapped by questions of this kind, but one can only guess at how much. We cannot tell which politicians the subject is reminded of, or the relative importance of most of the images conjured up by the statements. The goal of scholars in the field is to get below this surface veneer to tap deeper hostility.

To do this it would seem vitally important that the subject be stimulated to think about some more focused symbol than "politicians"[18] and that the items not be phrased in simple agree-disagree form. This will not completely avoid the problem of imperfect measurement due to ritualistic responses, but it should mitigate it somewhat by making the statement of disaffection more meaningful to the subject.

Stokes' scale is a model of what we seek here. Before the items are presented to the respondent he is told: "Now I'd like to talk about some of the different ideas people have about the government in Washington and see how you feel about them. These opinions don't refer to any single branch of government in general."[19] A series of items (see below for examples) follows which are not in agree-disagree form and which are reversed so that the positive alternative is not presented first in each case. These questions measure whether the respondent believes that the government generally does the right things, and whether it serves the public interest. The format is designed to cut down response-set problems and, in this case, focus the respondents' attention on the collective workings of an identifiable set of political arrangements and institutions.

As Gamson syas, "it is possible for individuals simultaneously to feel high confidence in political institutions and alienation towards those who man them."[20] In fact, according to Gamson, it is important to find out whether political trust is generalized—that is, in the simplest case, whether people dissatisfied with a given decision or set of decisions first begin to distrust the authorities, then perhaps the institutions and procedures of the regime, and finally become so disenchanted with the political community itself that they wish to separate themselves from the community. Where trust is high a negative decision may be bearable because of a belief in the integrity of the authorities and the legitimacy of

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personal trust, unrelated to political cynicism in Boston, is directly related to the expression of cynical comments about politicians in the suburban community."

[15] Stokes, *op. cit.*, p. 67.

[16] *Ibid.*, p. 64.

[17] McClosky, *op. cit.*, p. 370.

[18] See Aberbach, *op. cit.*, pp. 25–42 for a detailed discussion of the importance of specifying the focus in measuring disaffection and pp. 46–56 for a critique of the political trust literature using this perspective. A briefer discussion can be found in Joel D. Aberbach, "Alienation and Political Behavior," this REVIEW, 63 (1969), pp. 86–99. See, also, Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York: Harcourt, Brace & World, 1965), pp. 453–455.

[19] Inter-University Consortium for Political Research (ICPR), *1966 Election Study* (Ann Arbor, 1968), p. 129.

[20] Gamson, *op. cit.*, p. 19.

the procedures employed. Where trust is low, negative outputs may be unbearable and lead to an intensification of distrust or separatist feelings. If several sets of authorities prove unsatisfactory, citizens are likely to "conclude that the institutions themselves may be the source of bias, and 'throwing the rascals out' will have little effect if indeed it is even possible."[21] The existence of high levels of trust allows authorities to make commitments which build more trust and weather situations in which citizens are unhappy about governmental outputs. A distrustful citizenry, however, is suspicious of every perceived governmental move, impatient for results and prone to deeper and more extreme levels of distrust. Ultimately, this process may lead to acts which undermine the political system.

III. THE ORIGINS AND CONSEQUENCES OF POLITICAL TRUST

There are two general approaches to explaining political trust employed in the existing literature. Gamson, for example, emphasizes political factors. The content of decisions and the reactions they provoke are seen as the basic sources of political trust and distrust. Other scholars stress personality factors which are basically independent of political considerations as explanatory variables. They believe that: "If one cannot trust other people generally, one can certainly not trust those under the temptation of and with the powers which come with public office. Trust in elected officials is seen to be only a more specific instance of trust in mankind."[22]

Litt has introduced the idea that both political and personality variables are potentially important as explanatory factors. The relative importance of each class of variables, however, depends on the political environment or "milieu" prevailing in the community being examined. Stokes draws attention to the importance of generalized political expectations born of widely held democratic values as influences on political trust. Individuals may be influenced in their thinking by a local government's reputation for political corruption, and this seems to affect the relative influence of personality factors in explaining the existing degree of political trust. Trustful attitudes are also determined, however, by general public expectations about the nature of democratic governments, and a government's general record of performance in certain policy areas.

We do not suppose that levels of political trust are immutable. We conceive, instead, of a process in which this basic orientation toward the system[23] slowly changes as individuals are subjected to outside influences. In Gamson's model, for example, the individual's level of distrust is based on his judgement of the content of political outputs important to him and the procedures used to reach decisions. These judgements cumulate through time and are affected by cues from his experiences with government and his group allegiances. For a person with a high level of trust, a bad decision may be seen as an understandable, if unfortunate, mistake which does not call the political system's legitimacy into serious question. For a person with a moderate level of trust, however, the same bad decision is more likely to serve as proof of fundamental faults in the political system and may precipitate a rapid decrease in political trust. For the already distrustful person, the bad decision is merely further proof that the system is evil and may move him to some extreme, perhaps violent, protest.

There are numerous feedback loops in the complex process which generates or maintains trust. For example, satisfactory outputs stimulate trust, but trust itself predisposes a person to view outputs positively. The same process operates when we view trust or distrust as an element leading to radical ideologies or behavior. A distrustful person, for example, should be more disposed to take part in violent activities or to endorse radical interpretations of social ills than a trusting one, but his behavior or endorsement reinforces his distrust or tends to lower his previous level of trust.

We will need extensive time-series data to study this developmental process in detail and to determine precisely the levels of trust and the structural conditions which are sufficient to maintain a stable system or to inhibit the generalization of distrust. We are currently gathering data which we hope will carry us in this direction.[24] Many important questions, however, can be answered simply using data collected at one point in time, although this limits us to inferences about feedback loops and to primary reliance on summary measures (overall assessments gauged at one point in time) of people's satisfaction with their status in life and their political achievements.

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[21] *Ibid.*, p. 51. Gamson suggests a series of conditions which discourage the generalization of political distrust. Among them are the disaggregation of large issues into smaller ones, an emphasis on the ad hoc nature of decisions (so that citizens do not see in negative decisions the application of general rules or principles), and a structural situation in which memberships of groups with varying goals and experiences overlap.

[22] Robert E. Lane, *Political Life* (Glencoe: Free Press, 1959), p. 164.

[23] M. Kent Jennings and Richard G. Niemi discuss political trust in these terms on p. 177 of their article on "The Transmission of Political Values from Parent to Child," this REVIEW, 62 (1968), 169–184.

IV. THE DATA

Our data come from a survey of the residents of Detroit, Michigan, completed in the fall of 1967. A total of 855 respondents were interviewed (394 whites and 461 blacks). In all cases whites were interviewed by whites, and blacks by blacks. The total N came from a community random sample of 539 (344 whites and 195 blacks) and a special random supplement of 316 (50 whites and 266 blacks) drawn from the areas where rioting took place in July 1967.[25] When we discuss the attitude patterns in the communities we will use the random sample data. However, since there are few meaningful differences between the distributions and relationships in the random and riot-supplement samples, we have employed the total N in the analysis so that a larger number of cases are available when controls are instituted.

V. LEVELS OF POLITICAL TRUST: A RACIAL COMPARISON

We defined political trust, following Stokes' lead, as a basic evaluative orientation toward government. Our measure of trust is a revised version of his. The following questions were asked at various points in the questionnaire:[26]

1. How much do you think you can trust the government in Washington to do what is right: just about always, most of the time, some of the time, or almost never?
2. Would you say that the government in Washington is pretty much run for the benefit of a few big interests or that it is run for the benefit of all the people?
3. How much do you feel that having elections makes the government in Washington pay attention to what the people think: a good deal, some, or not very much?
4. How much do you think we can trust the government in Detroit to do what is right: just about always, most of the time, some of the time, or almost never?
5. How much do you feel having elections makes the government in Detroit pay attention to what the people think: a good deal, some, or not very much?

In the minds of Detroit residents there is a generalized sense of trust in the federal and local governments.[27] While trust in the Washington government on the individual items is always higher than trust in the Detroit government, the differences are slight. Detroit city government is relatively well run, nonpartisan, and generally not in such ill repute as the governments of cities like Boston or Newark.[28] This is apparent in Table 1 where we compare the levels of political trust exhibited by blacks in Newark and Detroit. It is clear that Litt is correct and the particular political setting is an important determinant of the level of trust. Since this is so, in the Detroit case we are fortunate to have an adequate distribution of responses to the attitude items so that we can examine the relationship between political trust and a general personality variable like trust in people which many scholars believe is the foundation of political trust under ordinary circumstances.

When we compare the political trust of blacks and whites (Table 2) we find that the blacks are less trusting. This holds for all of the individual items as well as the index as a whole. This actually represents a change in the usual pattern, as blacks have always had at least the same distri-

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[24] This paper is based on data gathered in 1967 in Detroit. In 1968 we re-interviewed a random subsample of the original sample ($N = 295$) and we will interview a larger number of respondents in 1970, many of them for the third time. Our study will also include interviews done in 1967 and 1970 with administrators in the Detroit city government and with business, civic and labor leaders who are members of the New Detroit Committee.

[25] Riot areas were defined by a location map of fires considered riot-related by the Detroit Fire Department.

[26] The wording of these questions is drawn from Survey Research Center questionnaires. Preliminary statements of the kind cited above were included. See ICPR, *op. cit.*, pp. 129–132.

[27] A single political trust index was constructed. The items formed a clear dimension when data from the study were factor-analyzed. The factor analyses (varimax rotation) were performed on the whole data-set and separately for blacks and whites. Questions on Detroit and Washington are equally weighted so that the index runs from 0 to 4.

[28] For confirmation of this view see: Edward C. Banfield, *Big City Politics* (New York: Random House, 1965), pp. 51–65; and David Greenstone, *Report on the Politics of Detroit* (Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University, 1961), Chapter 2.

TABLE 1. TRUST IN DETROIT AND NEWARK GOVERNMENTS FOR RIOT AREA BLACK MALES: 15–35*

Item; How much do you think you can trust the government in (Newark/Detroit) to do what is right: just about always, most of the time, some of the time, or none of the time?

	Trust City Government				
	Just about always	Most of the time	Some of the time	None of the time	%
Newark (N=232)	2%	9	50	38	=100%
Detroit (N=71)	10%	21	51	18	=100%

* The figures for Newark are recomputed from the table in the *Report of the National Advisory Commission on Civil Disorders* (New York: Bantam Books, 1968), p. 178. This survey, conducted for the Commission by Nathan Caplan of The University of Michigan, covered only males 15 to 35 living in the riot zone and we drew comparable respondents from our sample to facilitate comparison.

bution as whites on answers to these political trust questions.[29] No survey data exist concerning levels of trust which prevailed at earlier times, but through the years the federal government and local governments in much of the North, for all their shortcomings, have been the black man's friend in an otherwise hostile environment. The federal government, especially, won him his freedom, gave him the best treatment he received in his bleakest days in the South, provided relief in the Depression and in the difficult periods which have followed, and has done the most to secure him his rights and protect him during his struggle for equality.[30] In addition, the government in Washington has been the symbol of the American Negro's intense identification with and "faith in the American Dream."[31] Now, at least in cities like Detroit, this sense of trust is being undermined as many black people are beginning to reject their traditional ties with paternalistic friends and allies, and are striking out at the more subtle forms of discrimination and deprivation found in the North. These expressions of distrust, as we shall see in more detail below, are accompanied by a militant racial ideology and an expressed willingness to resort to almost any means necessary to achieve their goals.

VI. EXPLAINING POLITICAL TRUST

A. *As a Function of Trust in People.* As we have mentioned, one commonly held hypothesis about the origins of political trust is that it is "only a more specific instance of trust in mankind"[32]—which is a personality factor basically independent of political considerations. Our survey contained a standard version of the Rosenberg Trust-in-People measure[33] which should provide an excellent means of testing the relationship between interpersonal trust and political trust. Given the similarities in the concepts *and* the measures, in fact, anything short of a strong relationship would raise serious questions about the hypothesis and one would expect "personality" variables other than trust in people to show even weaker direct effects on political trust.

The relationship between the indicators of

TABLE 2. POLITICAL TRUST IN THE DETROIT BLACK AND WHITE COMMUNITIES

	Political Trust Index			
	Low 0–1	2	*High* 3–4	
Black Random Sample (N=186) $\bar{x}=1.66$*	52	13	35	=100%
White Random Sample (N=327) $\bar{x}=2.13$*	33	24	43	=100%

* The mean in the Black Riot Area Sample (N=341) is 1.67 and 1.66 in the Total Black Sample (N=461).

The mean in the White Riot Area Sample (N=75) is 2.12 and 2.11 in the Total White Sample (N=394).

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[29] See Stokes, *op. cit.*, pp. 61–73 and Aberbach (1967), *op. cit.*, pp. 119–126.

[30] For example, see William Brink and Louis Harris, *The Negro Revolution in America* (New York: Simon and Schuster, 1964), pp. 131 and 232–233 on Negro attitudes towards various political institutions and figures.

[31] Louis E. Lomax, *The Negro Revolt* (New York: Harper and Row, 1962), p. 250; and also see Gunnar Myrdal, *An American Dilemma* (New York: Harper and Row, 1944), pp. 3–5, 880 and 1007 on the Negro as an "exaggerated American."

[32] Lane, *op. cit.*, p. 164.

[33] See Rosenberg, *op. cit.* The version we used consists of two of the three questions regularly asked by SRC in their surveys. They are:

1. Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?
2. Do you think that most people would try to take advantage of you if they got a chance or would they try to be fair?

trust in people and political trust is positive but weak. Rank-order correlation coefficients (Gamma) between the two are .17 for blacks and .16 for whites.[34] Clearly, political trust is more than a mere specific instance of trust in mankind. A strong relationship between interpersonal trust and political trust would hold ominous implications for American race relations given the low level of trust in people which most studies have discovered among blacks. In our data, for example, over 50 per cent of the whites but less than 30 per cent of the blacks have high scores on our trust-in-people scale. The pattern of these differences hold with education controlled and was the same in the Michigan Survey Research Center's 1964 national survey where similar questions were asked.[35]

B. *As a Function of Social Background Factors.* A simple and plausible explanation of variation in political trust is that the socially advantaged are more trusting than the disadvantaged because they possess the status and the skills which bring them societal rewards and honors, while the disadvantaged achieve relatively little, and as a result, have little faith either in their fellow men or their government.[36] Our data forces us to reject this simple explanation. There is virtually no relationship between indicators of social advantages, such as education, occupation and income, and political trust.[37] If such factors have an effect, it is indirect.

There are other background factors, however, which have a greater influence on political trust. Individuals who were born in the South are somewhat more trustful than those born in the North[38] and people who have active affiliations with churches (i.e., are members of churches or church-related groups) are more trusting than those who are inactive.[39] These relationships are not strong, but reflect important acculturation patterns; persons born in the North more readily adopt a "worldly" cynicism about government, and individuals who have broken away from the traditional moorings of the church are also less likely to believe that government represents a benevolent authority. Our measures of this acculturation process are crude and indirect at best. In future studies we intend to create more explicit measures of this form of modernization which will enable us to ascertain more exactly the strength of its impact on political trust.

C. *As a Function of Political Expectations.* Since political trust does not seem to be merely a reflection of basic personality traits, or a simple product of social background, we turn to political factors in search of a more satisfactory explanation. As a person gains experience in the political realm, he slowly builds an assessment of himself as a political actor and develops his ideas about the fairness of the political process and the utility of its outputs. These evaluations are summaries both of his actual experiences and his expectations. They are answers to a series of questions:

(1) Am I, or can I be, influential?
(2) Do governmental outputs make a difference in my life—are they beneficial?
(3) Do I, or will I, receive equal treatment if I have a grievance about governmental decisions?

Each answer is an element in the political equation suggested by Gamson where political trust is a function of an individual's cumulative assessment of the procedures and outputs of the political process.[40] Also, if Stokes is correct, in a system infused with the democratic ethos, perceived influence is as important as the quality and justice of the outputs themselves in determining political trust.[41]

Obviously, political expectations are a complex function both of factors in an individual's personality and his assessment of a political situation. We cannot hope to sort out these elements in the attitudes we use as predictors, but we assume that political evaluations have strong foundations in the cumulative political experiences of an individual or group. In the process model we are developing, political experiences and expectations have a more immediate effect on political trust than personality factors, and are themselves conditioned by a respondent's level of trust. Because of these feedback relationships, we would expect individuals' political experiences and expectations and their feelings of political trust to be strongly associated. We do not assume that all governmental decisions will affect political trust, but we are struck by Gamson's idea that "disaffection begins to be generalized when an undesirable outcome is seen as a member of a class of decisions with similar results."[42]

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[34] See Aberbach, *op. cit.* (1969), pp. 92–93 for somewhat similar findings for whites using 1964 SRC national sample data.

[35] Aberbach, *op. cit.* (1967), pp. 104–114.

[36] The notion of a "theory of social disadvantages" as a general explanation for attitudes of estrangement is developed at length by Marvin E. Olsen, "Political Assimilation, Social Opportunities, and Political Alienation" (unpublished Ph.D. Dissertation, The University of Michigan, 1965).

[37] For example, the correlation (Gamma) between education and political trust is .08 for blacks and .03 for whites.

[38] The correlation (Gamma) between regional birthplace and political trust is .14 for blacks and .13 for whites.

[39] The correlation (Gamma) between active affiliation with a church and political trust is .24 for blacks and .15 for whites.

[40] Gamson, *op. cit.*, p. 51.

[41] Stokes, *op. cit.*, p. 67: "When the individual's sense of political efficacy is compared with his positive or negative attitude toward government, it is apparent that a sense of ineffectiveness is coupled with feelings of hostility. This relation is more than a tautology. In other cultures or other historical eras a sense of ineffectiveness might well be associated with a positive feeling. In the context of democratic values, feelings of powerlessness toward public authority tend to create feelings of hostility toward that authority."

In order to investigate these relationships we have utilized measures of our respondents' sense of political competence, their beliefs about the importance of the actions taken by government, and their expectations concerning the kind of reception they would receive at a government office. (The questions employed and the methods used in constructing indices are listed in the footnotes to Table 3.) A look at Table 3 will show that each of the political indicators is much more strongly related to political trust than was our measure of trust in people. These are substantial relationships, indicating the power of a political explanation. Controlling for education does not change the basic picture in two of the three cases, even though a variable like political competence is strongly related to education. However, the effect of education on the gamma between trust and expectations of equal treatment is worth further discussion.

First of all, a feeling that one would receive worse treatment than other people in attempting to solve a problem at a government office is more strongly related to political trust for whites than for blacks. Naturally, there are more blacks who expect unequal treatment (35 percent versus 12 percent of the whites), but if we correctly gauge the intensity of emotion reflected in answers to this question in our survey, white respondents who believe they would receive a harder time at a government office are even angrier than blacks with similar expectations. Many of the whites are evidently convinced that blacks receive special treatment and are given favors without deserving them. One typical white respondent said he would be given unequal treatment

> "because the white people are discriminated [against]. If you have a home and are working and you have pride, they just don't come to your assistance. One who has no will power or pride, they'll give you assistance."[43]

For the minority of whites with such extreme views (especially those in the lower education group), the emerging assertiveness of blacks is clearly a factor of the utmost importance in determining their level of political trust.

Expectations of treatment in a government office is of less direct importance for blacks, especially the upper educated group. This is not because all upper status blacks expect equal treatment (25 percent do not), but results from the fact that there is a relatively flat distribution of political trust scores across categories of our expectations-of-equal-treatment measure. One possible explanation, which is supported by data we will now present, is that distrust among upper status blacks does not arise so much from actual or expected discrimination as from empathy for others in the black community who experience these insults in worse form. The stronger relationship for lower education blacks indicates a more direct effect of expected discrimination on trust for this group, but even here the effect is weaker than for whites. Something more than blatant personal mistreatment underlies black political distrust.

D. *As a Function of Feelings of Deprivation.* There is a large literature concerning the relationship between psychological deprivation and political unrest (denied as violence or propensity to engage in violent behavior)[44] and a de-

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[42] Gamson, *op. cit.*, p. 51.

[43] This quote is from one of the respondents included in our 1968 panel. The 1967 questionnaire did not probe answers to the close-ended question on equal treatment in a government office. After examining the 1967 interview protocols, we believed that whites who felt they would receive unequal treatment often ascribed this to reverse discrimination and we used the 1968 interviews to confirm this hypothesis.

A poll by the Gallup organization reported in *Newsweek* (October 6, 1969) gives evidence of somewhat similar feelings among "a substantial minority of whites" that "the black man already has the advantage." (p. 45).

[44] For examples of analyses employing aggregate data see Ivo K. Feierabend, Rosalind L. Feierabend, and Betty A. Nesvold, "Social Change and Political Violence: Cross-National Patterns" and James C. Davies, "The J-Curve of Rising and De-

TABLE 3. CORRELATIONS (GAMMA) BETWEEN POLITICAL EXPECTATIONS AND POLITICAL TRUST, BY RACE AND EDUCATION

Political Experiences and Expectations	Blacks			Whites		
	Zero-Order	Low* Education	High* Education	Zero-Order	Low* Education	High* Education
Political Competence**	.40	.37	.45	.32	.30	.44
Impact of Governmental Actions***	.40	.42	.43	.32	.34	.21
Expectations of Equal Treatment****	.26	.32	.13	.42	.51	.33
(N) =	(461)	(322)	(122)	(394)	(254)	(124)

* Respondents in the low education group include all individuals who have completed high school, while those in the high education group have, at minimum, gone beyond high school to either special training or college. These definitions of low and high education are retained throughout the paper. The N's for each group are also the same. We chose education as a status indicator and dichotomized the sample in order to preserve the maximum number of cases for the analysis.

** The following items were used in the political competence index:

1. How much political power do you think people like you have? A great deal, some, not very much, or none?
2. Suppose a law were being considered by the Congress in Washington that you considered very unjust or harmful. What do you think you could do about it?
   - 2a. If you made an effort to change this law, how likely is it that you would succeed: very likely, somewhat likely, or not very likely?
3. Suppose a law were being considered by the common council that you considered very unjust or harmful. What do you think you could do about it?
   - 3a. If you made an effort to change this law, how likely is it that you would succeed: very likely, somewhat likely, or not very likely?

*** How much difference do you think it makes to people like yourself what the government in Washington does? A good deal, some, or not very much?

**** Suppose that there was some question that you had to take to a government office—for example, a tax question, a welfare allotment, or a housing regulation. Do you think that most likely you would be given a harder time than other people, would be treated about the same as anyone else, or would be treated a little better than most people.

All variables are coded so that positive experiences and expectations receive high scores.

veloping literature, using aggregate data, which speculates about the relationship between deprivation and feelings that the government is not legitimate.[45] One of the best psychological measures of deprivation now available is the Cantril Self-Anchoring Scale which indicates the discrepancy between an individual's definition of the "best possible life" for him and his past, present, or future situations.[46] After each

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clining Satisfactions as a Cause of Some Great Revolutions and a Contained Rebellion," pp. 632–688 and 690–731 respectively in Hugh D. Graham and Ted R. Gurr (eds.), *The History of Violence in America* (New York: Bantam, 1969). An example of the use of psychological data is Don R. Bowen, Elinor Bowen, Sheldon Gawiser and Louis H. Masotti, "Deprivation, Mobility and Orientation Toward Protest of the Urban Poor," pp. 174–187 in Louis H. Masotti and Don R. Bowen (eds.), *Riots and Rebellion: Civil Violence in the Urban Community* (Beverly Hills: Sage Publications, 1968).

[45] A particularly interesting analysis of this type which is used to speculate about urban unrest in the United States is found in Ted Gurr, "Urban Disorder: Perspective from the Comparative Study of Civil Strife," pp. 51–69 in Masotti and Bowen, *op. cit.* More details on the measures used in Gurr's study can be found in Ted Gurr, "A Causal Model of Civil Strife: A Comparative Analysis," this REVIEW, 62 (1968), 1104–1125.

[46] See Hadley C. Cantril, *The Pattern of Human Concerns* (New Brunswick: Rutgers University Press, 1965). Our respondents were given the following set of questions:

> Now could you briefly tell me what would be the best possible life for you? In other words, how would you describe the life you would most like to lead, the most perfect life as you see it? (Show R card with a Ladder.)
>
> Now suppose that the top of the ladder repre-

respondent gives a definition of the life "he would most like to lead" in his own words, he is shown a picture of a ladder with ten rungs and asked to imagine that the top rung represents the best possible life which he has just described. He is then asked to rank, in comparison with his ideal, his present life, his life five years ago, and what he expects his life to be like five years in the future. A person's position on these scales is a function of his own definition of the best possible state of affairs. His standards may be determined by class or race models, or expectations created by the mass media, but no simple objective indicator of achievement like income or occupation will be an adequate substitute for this psychological measure.[47]

We use these measures of deprivation as indicators, based on standards meaningful to each individual, of a deep-rooted dissatisfaction or expectation of dissatisfaction which may be blamed on government. They are conceptually and empirically independent of the *political*-expectations indices employed in the previous section since a person may be deeply dissatisfied with the general course of his life, but feel politically powerful, believe he receives equal treatment from government and feel that governmental outputs have a beneficial impact. In other words, the two sets of indicators are related, but do not have the same psychological significance for the individual, and each has an independent effect on political trust.[48]

We employed two sets of self-anchoring scales in our surveys: One was the standard "best possible life" question explained in note 46 and a second sought the respondents' definitions of the "best possible race relations" as a base for selecting rungs on a ladder running from 0 to 10.[49] Both whites and blacks gave a wide variety of definitions of the "best possible life" in response to that question, with almost none of them directly involving race relations. While whites were much more satisfied with their past and present lives, both racial groups are strongly optimistic about the future.[50] When we turn to race relations we find very substantial differences in the patterns of answers. Blacks talk almost exclusively in terms of total integration, better personal relationships with whites, the

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sents the best possible life for you, the one you just described, and the bottom represents the worst possible life for you.

"Present Life" A. Where on the ladder do you feel you personally stand at the present time?

"Past Life" B. Where on the ladder would you say you stood five years ago?

"Future Life" C. Where on the ladder do you think you will be five years from now?

[47] In our study, for example, income is correlated (Gamma) .29 for whites and .23 for blacks with position on the "present life" ladder. Income is thus a meaningful predictor, but these are far short of simple one-to-one relationships.

[48] This proposition was tested for each racial group by a multiple regression analysis in which the measures of trust in people, the background factors, political experiences and expectations, and the ladders were used as predictors of political trust. The political variables and the relevant ladders each had an independent effect on trust with all of the other variables controlled. Multiple R's were .52 for the blacks and .49 for the whites.

[49] See footnote 46 above for the wording on the "best possible life" questions. The "best possible race relations" items were in the same form with the following sentences as the initial stimulus:

> Here in Detroit, as in many places, different races of people are living together in the same communities. Now I would like for you to think about the very best way that Negroes and white people could live in the same place together. In other words, what would be the very best kind of race relations, the most perfect you could imagine?

This item was adapted from that used by Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace and World, 1966), pp. 285–294, 513–514.

[50] Income and job advancement were desired by 28% of the blacks and 16% of the whites, good health or family life by 22% of the blacks and 20% of the whites and personal property (homes, cars, etc.) by 15% of the blacks and 11% of the whites. The major difference was that 13% of the whites (as opposed to 3% of the blacks) said the life they were now living was the best possible and 23% of the whites, compared with 9% of the blacks, mentioned peace and tranquility.

% Scoring High (7–10) on "Best Possible Life" Ladders, by Race

	Past Life	Present Life	Future Life
Blacks	13%	23%	64%
Whites	49%	47%	66%

We will present more complete descriptions and analysis of the answers to the "best possible life" question in Joel D. Aberbach and Jack L. Walker, *Race and the Urban Community* (Boston: Little Brown, forthcoming).

TABLE 4. CORRELATIONS (GAMMA) BETWEEN LADDER POSITIONS ON SELF-ANCHORING SCALES AND POLITICAL TRUST, BY RACE AND EDUCATION

Scales*	Blacks			Whites		
	Zero-Order	Low Education	High Education	Zero-Order	Low Education	High Education
"Best Possible Life" Ladders						
Past Life	.18	.27	−.12	.02	.06	.03
Present Life	.31	.39	.05	.20	.23	.20
Future Life	.30	.38	−.11	.15	.14	.13
"Best Possible Race Relations" Ladders						
Past Race Relations	.16	.22	.04	−.04	−.07	.00
Present Race Relations	.13	.23	−.17	.26	.29	.17
Future Race Relations	.10	.17	−.11	.37	.40	.29

* The ladders were trichotomized as follows: 1–3 = 0; 4–6 = 1; 7–10 = 2. (This is the division used by Cantril, *op. cit.*, p. 257). Therefore, a positive coefficient indicates that the higher a person's score on the various ladders, the higher his trust in government.

disappearance of color consciousness, and respect and dignity for all, while more than 30% of the whites spontaneously endorse segregation or separation of some kind. In addition, many more whites than blacks are pessimistic about the future in this area[51] with the correlation between expressed separationist feelings and whites scores on the future ladder at (Gamma) .40.[52] The white community, not the black, is divided over the desirability of integration and whites are more depressed than blacks about the prospects for future race relations in Detroit.

This fact is reflected in Table 4 which indicates how much more potent a predictor of political trust the present and especially the future race relations ladders[53] are for whites than for blacks. The situation is reversed when we look at the "best possible life" ladders. Here the correlations are higher for the black sample. In addition, for whites, controlling for education has only mild effects on the relationship between the ladders and trust (the high education group is somewhat more homogenous on the ladders than the low education group), but it substantially increases the correlations in the low education black group. The signs are actually reversed in four of six cases for the high education black group where those who are dissatisfied are actually more trusting than those who are satisfied.

If we look back at the discussion of the correlation in Table 3 between expectations of equal treatment and political trust we recognize certain similarities to the relationships we are now describing; in both cases a racial question is a better predictor of trust in the white commu-

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[51] Our "Best Possible Race Relations" ladders yielded the following results:

% Scoring High (7–10) on "Best Possible Race Relations" Ladders, by Race

	Past Race Relations	Present Race Relations	Future Race Relations
Blacks	10%	22%	61%
Whites	39%	23%	40%

[52] The correlation (Gamma) is .09 for blacks because there is virtual unanimity in the black community on integration. See Joel D. Aberbach and Jack L. Walker, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan," this REVIEW, 64 (1970), p. 883.

[53] Ted Gurr stresses the importance of "anticipated interference with human goals" in his analysis of discontent. He says that,

> analysis of the sources of relative deprivation should take account of both actual and anticipated interference with human goals, as well as of interference with value positions both sought and achieved. Formulations of frustration in terms of the "want:get ratio," which refers only to a discrepancy between sought values and actual attainment, are too simplistic. Man lives mentally in the near future as much as in the present. Actual or anticipated interference with what he has, and with the act of striving itself, are all volatile sources of discontent.

See p. 254 of Ted Gurr, "Psychological Factors in Civil Violence," *World Politics*, 20 (1968), 245–278.

nity, and in both cases the upper education black group is quite different from the lower education black group. In summary:

1) Blacks are less likely than whites to lose faith in the government when they expect discriminatory treatment in a government office or when they see failures in achieving the pattern of race relations they favor. For high education blacks there are even cases when the relationship between the race relations ladders and political trust is negative.
2) Lower status blacks tend to be very bitter about government when they fail to achieve their personal goals in life while higher status blacks do not.
3) While higher status black people are somewhat more satisfied and less discriminated against than lower status blacks, this is not enough to account for the differences in relationships found here since there is a fairly uniform level of trust no matter how poorly the higher status person expects to be treated or how deprived he feels. In fact, the deprived high education black person is likely to be a little more trusting than those in the same group who are relatively satisfied.

TABLE 5. CORRELATIONS (GAMMA) BETWEEN (A) PERSONAL EXPERIENCES OF DISCRIMINATION AND (B) RECOGNITION OF SERIOUS COMMUNITY PROBLEMS AND POLITICAL TRUST FOR BLACKS, BY EDUCATION

	Zero-Order	Low Education	High Education
(A) *Personal Experiences*			
Personal Experiences of Discrimination Index (Police Excepted)*	−.15	−.18	.00
Personal Experiences of Police Mistreatment**	−.43	−.57	−.21
(B) *Recognition of Serious Community Problems*			
Crowded Conditions	.02	.05	−.22
Poor Education	−.01	.00	−.21
The Way the Police Act	−.21	−.21	−.24

* This is a simple additive index of personal experiences of discrimination in Detroit in obtaining housing, in the schools from a landlord, or in obtaining, holding or advancing on a job.

** This is an index of reports of police mistreatment experienced by the person himself.

A negative coefficient indicates that the more a person has been discriminated against, mistreated, or recognizes a community problem as serious, the lower his trust in government.

The data for the white community are relatively easy to interpret: racial issues, especially those involving integration and governmental treatment of blacks become so important that they have superseded considerations of personal achievement, especially for the lower status group. Some of this may be due to the fact that our survey was conducted soon after a major disturbance, but large numbers of whites are clearly upset about the future of race relations and some actually feel discriminated against because of their race.[54] Government officials are faced with an increasingly angry, bitter and frightened group of white people who feel persecuted and unrepresented.[55] These feelings are undermining their basic trust in government and making them much more sympathetic to political candidates who call for repression of the blacks in the name of law and order.

A more complex process is at work in the black community. In Table 5 we see that the indicators of reported discrimination are differentially related to political trust for the lower and upper education segments of the black sample. Experiences of discrimination in obtaining housing or on the job are associated with distrust for the lower education group, but not the upper education group. Even police mistreatment, the most volatile issue in Detroit's black community, is much more strongly related to political distrust in the lower education segment of the population. (This is not because only lower status blacks experience mistreatment, since about 15 percent of each group report some form of bad experience.) However, when the issue is simply whether a community problem is recognized as important or not, the relationship between recognition and distrust is stronger in the

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[54] We have already seen above that there are some whites who believe that they would receive unequal treatment at a government office because of their race. Even more astounding, however, is the fact that in our 1968 survey of a random subsample of the original (1967) sample 46% of the whites believed that if they were black they would be either making advances toward their goals in life or advancing more rapidly toward their goals. This compares to 57% giving similar answers in the black community. Unfortunately, this question was not on our 1967 questionnaire.

[55] More than half of the white respondents in our sample could not name any national or local leader who represented their views on race relations and whites actually scored lower than blacks on our measure of subjective political competence. See our discussion of these points in Joel D. Aberbach and Jack L. Walker, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan," a discussion paper issued by the Institute of Public Policy Studies, The University of Michigan, 1968, pp. 27–34.

upper education group. Their distrust, unlike that of the lower education respondents, may not be rooted so much in concerns about *personal* experiences of expectations, nor even in considerations of larger and more abstract feelings about the conditions of race relations in Detroit, but in *empathy*—a feeling of identification with the black political and social community which includes persons from all social classes. This is part of a group identification gaining momentum in the middle class which identifies the fortunes of the black community, rather than prospects of the individual, as the key in evaluating decisions and institutions.[56] This could be the reason that our ladder measures of personal achievement are so successful as predictors for the lower education group and yet so unsuccessful for the upper education group. If some upper status blacks are identifying with others in the community who are persecuted, we would expect segments of both the lower and upper status groups to share a racial ideology of protest which is related to feelings of political trust. We will test this proposition in the next section.

VII. POLITICAL TRUST AND RACIAL IDEOLOGY

Our data give clear evidence of a developing racial ideology in Detroit's black community.[57] The elements of this belief system include a favorable interpretation of black power, the choice of militant black leaders as representatives of one's own point of view on race relations and a revolutionary interpretation of the meaning or significance of the 1967 disturbances. Scholars studying other cities have reached similar conclusions.[58] This ideology is not a manifestation of growing sentiments for separation,[59] but of a militantly expressed ideology of protest which demands quick and effective action to better conditions for *all* black people. Unfortunately, it is opposed by an equally militant ideology held by a large segment of the white community which demands racial separation and the curtailment of programs designed to aid disadvantaged blacks. These are the kinds of emotional issues which destroy trust in government and undermine the normal constraints on intemperate or even violent political behavior.

We will now examine the relationship between elements of these belief systems and political trust. Here, even more than in the previous section on explanations of political trust, we are dealing with a process in which a set of beliefs influences the level of trust which in turn influences or deepens the beliefs; the man who sees the 1967 riot as a justified reaction to social injustice is more likely to develop or sustain distrust of the government, but, in a cumulative spiral, this distrust strengthens his belief in the justification of the riot as a reaction to oppression. Since we are measuring these phenomena at a single point in time we cannot give our process model an adequate empirical test. Before progress can be made in verifying and refining our model, data on the same individuals must be collected on several different occasions.

Bearing in mind the restrictions placed on our efforts by the nature of our data, we turn first to our black respondents (Table 6) and see that each of the elements we have measured in the developing racial ideology is related to political trust. Blacks who label the 1967 disturbances as a revolutionary protest against mistreatment, favorably interpret the black power slogan, or select a militant as the leader best representing their views on relations between the races, are also likely to distrust the government. These

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[56] Lupsha has discussed the same basic phenomenon: "Anger can occur without one's being frustrated or deprived. One can learn that certain events, or violations of one's rights and values, should be responded to with hostility. One can be angry and aggressive because one's values or sense of justice (a learned phenomenon) have been affronted, without any blocking of the individual's goal-directed activity, or awareness of any personal "want-get ratio" deprivation, or any personal feelings of "anticipated frustration." One can be angry and aggressive simply because one believes the behaviors of the situation are wrong or illegitimate." See p. 288 of Peter A. Lupsha, "On Theories of Urban Violence," *Urban Affairs Quarterly* (1969), 273–296.

[57] Aberbach and Walker, *op. cit.* (1970), pp. 379–386.

[58] See, for example, T. M. Tomlinson, "The Development of a Riot Ideology among Urban Negroes," *American Behavioral Scientist* (1968), 27–31.

[59] Less than 2 percent of our black sample endorsed the idea of the separation of the races. This is not surprising in light of the history of the concept integration as a symbol of equality in the black community. We used the word separation in our questions in order to overcome the obvious connotations of segregation, but few of our respondents were attracted by the term and almost none used it spontaneously in their definitions of the "best possible race relations." Even among intellectuals, most of the debate about race relations revolves around various forms of social pluralism as opposed to assimilation. One of the major goals of our panel study is to examine the ways in which people modify their ideals about desirable forms of race relations and community goals through time. See Aberbach and Walker, *op. cit.* (1970), p. 383, especially footnote 49.

TABLE 6. CORRELATIONS (GAMMA) BETWEEN MILITANT IDEOLOGY AND POLITICAL TRUST FOR BLACKS, BY EDUCATION

	Zero-Order	Low Education	High Education
Favorable Interpretation of "Black Power"*	−.39	−.39	−.37
Favorable Explanation of the July, 1967, Disturbance**	−.22	−.20	−.25
Leader Best Representing the Respondents Views on Relations Between the Races***	−.23	−.19	−.32

* Favorable interpretations of black power (given a high score on this index) consist almost exclusively of notions about a "fair share" for blacks or "racial unity" in the black community as a tactic in bettering conditions. See Aberbach and Walker, *op. cit.*, for an extensive discussion of this.

** This is an index in which a high score indicates a revolutionary label for the disturbance and a belief that those who took part did so not because they were riffraff or criminals, but because they had been mistreated by society.

*** Respondents selected, without any cues from the interviewer, the leader who "best represented" their views on relations between the races. The selections were then scored from militant black leaders (high) through to conservative white leaders. See Aberbach and Walker, *op. cit.*, p. 385, for distributions.

sentiments are not a function of social status and, as we can see, the relationships are as strong or stronger for the upper educated black group as for blacks with lower levels of educational achievement. We should emphasize that these relationships are quite strong when we take into account the fact that the elements in the black ideology are all measured with open-ended survey questions. It is possible, therefore, to speak as we did before of a *black political community,* crossing social class lines, marked by a developing racial ideology focused on militancy and pride and connected with a strong distrust of government. This growing solidarity is a political phenomenon of the utmost importance for a minority community which needs to mobilize the skills of its growing middle class.

Turning now to the white community, we again find that racial variables are of great importance as predictors of trust, only here views on integration versus separation and spending public money to improve conditions in the ghetto are key factors in determining the level of trust. Whites are almost evenly divided on these issues (about 50 per cent of our white sample favor integration and approximately the same percentage endorse spending more money).[60] Such an overwhelming majority of blacks (over 90 per cent) favor both, however, that analysis of the correlates on these questions is not very fruitful.

TABLE 7. CORRELATIONS (GAMMA) BETWEEN ATTITUDES ON INTEGRATION, PUBLIC EXPENDITURES AND POLITICAL TRUST FOR WHITES, BY EDUCATION

	Zero-Order	Low Education	High Education
Integration*	.28	.29	.28
More Money for Improvements**	.35	.31	.39

* This is a summary index of responses to items calling for the endorsement or rejection of a general policy of integration or separation, school integration and the description of the "best possible race relations" coded according to the degree of integration or separation endorsed. We used the word separation in preference to segregation to insure that black respondents could comfortably endorse this alternative; only 2% did so.

** The following close-ended question was asked in the middle of our section on the riot and conditions in the black community: "Do you feel that more money or less money should be spent on trying to improve conditions?"

The racial issue and the means of dealing with it inspire great emotion in the white community and threaten to undermine trust in the government for a substantial segment of the population. As Table 7 demonstrates, separationists and those opposed to spending more money to improve ghetto conditions are decidedly more distrustful of government than integrationists and those willing to spend more money. The relationships hold for those with high levels of educational achievement as well as those with lower levels of education. In addition, attitudes on integration and scores on the race relations ladders have independent effects on the level of trust,[61] so that, for example, integrationists who are dissatisfied with the current or emerging course of race relations are more distrustful of the government than those who are satisfied, and they are also more distrustful than satisfied or optimistic segregationists. It is easy to envi-

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[60] The correlation (Gamma) between the two is .49 for whites. The exact distributions by race on spending public money are as follows:

Spend More Money to Improve Conditions

	More	Same	Less	DK, NA	
Blacks	94%	4	1	1	=100%
Whites	50%	28	19	3	=100%

[61] For example, the correlation (Gamma) between political trust and scores on the future race relations ladder is .36 for segregationists and .27 for integrationists. It is .37 for the entire white sample.

TABLE 8. POLITICAL DISTRUST AND POTENTIAL POLITICAL BEHAVIOR FOR BLACKS AND WHITES

	Blacks			Whites		
Political Trust	Can you imagine a situation in which you would riot?*			If the election for mayor of Detroit were held tomorrow and the candidates were Jerome Cavanaugh and Mary Beck, who would you vote for?		
	Yes or Maybe	*No*	(N)	*Beck*	*Cavanaugh*	(N)
Low 0	54	46	(129)	74	25	(42)
1	35	65	(86)	42	58	(36)
2	35	65	(94)	52	48	(60)
3	21	79	(75)	39	61	(46)
High 4	17	83	(59)	26	74	(69)
	Gamma = .40			Gamma = .35		

(Percentages are across.)

* The word "riot" was not actually used. Respondents were asked early in the interviews to give their own label to the events of July, 1967, and this term was used throughout by the interviewer.

sion situations in which events or governmental policies and pronouncements embitter both segregationists and integrationists, thereby dealing a double blow to the level of political trust in the white community.

VIII. POLITICAL DISTRUST AND POLITICAL BEHAVIOR

Distrust of the government creates a tension in the polity which can build for some time, but ultimately seeks release. Among other things, people can revolt, engage in limited displays of violence like riots, demonstrate, or support candidates for elective office who give voice to their fears and frustrations. The mode of expression depends on the depth of the discontent, traditions of violence in the society, loyal coercive forces available to the government, and the availability of free electoral processes.[62] At this point many distrustful blacks have taken to the streets and distrustful whites troop to the polls to vote for so-called "law and order" candidates.

We asked respondents whether they could "imagine any situation" in which they would take part in a disturbance like the one Detroit had in July of 1967 and we also conducted a mock mayoral election in which the choice lay between the incumbent mayor (Jerome Cavanaugh) and a very vocal member of the Detroit Common Council (Mary Beck) who had been courting backlash support. Since very few whites could envisage taking part in a disturbance, of the 1967 variety at least, and few blacks would ever vote for Miss Beck, we could only employ each indicator for one racial group. This is simply a matter of convenience. We certainly do not wish to imply that blacks would never vote for extremist candidates or that whites would never engage in violence.

As Table 8 indicates, distrustful whites are indeed strongly in favor of Miss Beck and distrustful blacks are better able to imagine situations in which they would riot. Distrust clearly stimulates a willingness to engage in violence or favorably predisposes people toward voting for extremist candidates. Moreover, high levels of trust serve to dampen the behavioral impact of adverse experiences while low levels of trust lead to volatile situations in which each insult increases the probability of extreme behavior. In statistical terms, political distrust and adverse experiences interact.

A classic example of this interaction can be seen in the Detroit black community where political distrust and reported experiences of discrimination interact to inspire willingness to engage in a civil disturbance (See Table 9). When trust is low, experiences of discrimination have a very powerful effect on a person's ability to imagine a situation in which he could take part in a civil disturbance, but high trust seems to serve as a dike which blunts somewhat the political effects of these experiences. Persons who are low in trust seem to interpret each experience of discrimination as further proof that the political system is evil and must be dealt with by any means, while those who are trusting have a less severe reaction to these experiences. High levels of trust are resources which governments can use to gain time in order to correct wrongs in

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[62] Gurr, "Urban Disorder: Perspectives from the Comparative Study of Civil Strife," *op. cit.* See Aberbach, *op. cit.* (1969) for an extended discussion of political distrust and political behavior.

TABLE 9. EXPERIENCES OF DISCRIMINATION AND WILLINGNESS TO TAKE PART IN A CIVIL DISTURBANCE BY LEVEL OF POLITICAL TRUST FOR BLACKS*

Trust	Low (0–1)			High (2–4)		
Reported Experiences of Discrimination	Can you imagine a situation in which you would riot?					
	Yes or Maybe	*No*	(N)	*Yes or Maybe*	*No*	(N)
Few (0–1)	30	70	(122)	22	78	(139)
Medium (2)	60	40	(31)	30	70	(37)
Many (3–4)	71	29	(61)	36	64	(44)
	Gamma = −.65			Gamma = −.26		
	(Percentages are across.)					

* This is a simple additive index of reports of personal experiences of discrimination in Detroit in obtaining housing, in the schools, from a landlord, or in obtaining, holding, or advancing on a job.

the society. When trust is low injustices have a stronger and more immediate impact since the reservoir of good will has been destroyed.

IX. POLITICAL TRUST AND RACIAL IDEOLOGY: AN OVERVIEW

We conceive of political trust as a central element in a dynamic process. Earlier research has shown that the most important variables which influence trust differ somewhat according to the political environment in which the research takes place, and our own analysis demonstrates that determinants of trust differ from group to group. Since our study is based on data collected from only one city we cannot claim universal applicability for our findings, but we believe that data collected in Detroit have characteristics which make them eminently suitable for studies of political trust. Data from our Detroit sample, unlike those gathered in cities like Boston or Newark which have reputations for corruption and inefficiency, are distributed in such a way that legitimate examinations of the relationships between political trust and several of its possible determinants can be conducted successfully.

Our analysis shows that political trust is not merely a reflection of our respondents' basic personality traits, or a simple function of general social background factors. Our most important explanatory variables are those which arise from the workings of the social or political system, such as the citizen's expectations about the treatment he will receive from government officials, general feelings of deprivation and well-being, and beliefs about the status or acceptability of one's group in society. Levels of trust are determined by these factors and, in turn, are influenced by them in a chain of interactions which continues as the political system operates. Repeated setbacks or disappointments are necessary to dissipate trust when it is high, and when trust is low, numerous successes are needed to increase it. Gamson argues that when a group has become extremely disaffected it is especially difficult to regain their trust because they may see any concessions made to them merely as proof that a corrupt system responds only to threats.[63] Our analysis lends support to many of Gamson's propositions, but his explanation of changes in levels of political trust cannot be given a conclusive empirical test until data are collected at several points in time from the same respondents. We are now collecting such data from our Detroit sample and will report our results in later work.

The determinants of trust are not the same for every social grouping within our sample. Among lower status blacks, high political distrust is related to expectations of discriminatory treatment in contacts with government officials, low feelings of political competence, experiences of racial discrimination, complaints about arbitrary or unjust behavior by the police, and low evaluations either of one's present situation in life or one's personal future prospects. Those lower status blacks who believe themselves to be suffering from some form of deprivation tend to become distrustful of the political system.

For upper status blacks, on the other hand, adverse experiences and dissatisfaction with their current achievements do not necessarily lead to political distrust. In fact, the small number who are dissatisfied with their present lives are slightly more trusting than those who are satisfied. Political distrust is highest among those middle class blacks who have a strong sense of identification and empathy with the problems of the black community. The accommodating, apolitical "black bourgeoisie" described by E. Franklin Frazier[64] is fast disappearing. The emerging concern with the nature of black identity or the meaning of the black heritage are reflections of the desire of blacks to find places of dignity and respect in the American social system. No matter how satisfied or dissatisfied they may be with the courses of their own private lives, many middle class blacks increasingly feel bound up with the black community and its problems. They are anxious to obtain assurances that the society will recognize the legitimacy and value of their culture and life style. They look to the government's policies and to the society's ceremonial or ritual acts for evidence that their status and importance is being recognized.

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[63] Gamson, *op. cit.*, pp. 172–178.

Political distrust among whites is rooted in their resentment and dissatisfaction with the course of American race relations and in a sense of political powerlessness. Our interview protocols are filled with bitter, angry outbursts, especially from the lower status whites, who believe they are victims of a cynical government which is willing to grant any black demand in exchange for votes and popularity. Most of the concessions being made, especially the symbolic gestures, seem to come at their expense. They feel leaderless and powerless in this new, confusing environment. In the past, their self respect and social orientation have been founded on myths of racial superiority which are being destroyed in the social revolution now taking place. Behind their anger we detect a sense of profound bewilderment and fear. White anxieties may be expressed in support of political leaders who seem to recognize their dignity and promise a return to "law and order," but they are as much victims as oppressors. Whatever our view of them, however, these restless whites are a potentially powerful political element which must be taken into account when public policies are being developed.

X. POLICY IMPLICATIONS

Emerging from our analysis are the outlines of an ominous confrontation between the races. The growing sense of solidarity and racial identification among blacks is being matched by rising, increasingly bitter resentment among elements of the white community. More often than in past decades, the anger and resentment of both sides is being translated from generalized racial hostility into focused political demands for specific programs or policies from agencies of both local and national government. These developing tensions may precipitate the kinds of social changes being called for by blacks, but they could lead to an altogether different result. If both black and white citizens lose sufficient confidence in the essential trustworthiness of the government, the society may reach its political "tipping-point," contraints on intemperate or even violent protest may completely disappear, and the stage will be set either for large-scale anti-democratic efforts at change by blacks and/or massive attempts at reaction and repression by whites.

The government's success in avoiding a complete break-down in race relations depends, to a significant degree, on its success in building political trust. Declining trust can be a stimulant to social change. The direction or nature of the changes, however, will be determined by the reactions of the government or the rest of society to the demands of distrustful groups. If public officials are able to build or maintain high levels of trust, a broader range of policy options are opened for consideration and governments can more easily risk short term opposition from some groups in the hope of achieving an important long term result. When trust is low, however, groups are unlikely to give the government the benefit of the doubt and may begin to call for immediate fulfillment of their demands. A dangerous process of competitive mobilization may begin. In Gamson's words: "The presentation of demands by one group stimulates their presentation by others. Thus, it is possible for the loss of trust to encourage a 'deflationary' spiral akin to a run on the bank."[65] The level of trust, in other words, determines the amount of patience or forebearance citizens can be expected to exercise. Since the problems of finding an equitable and peaceful new basis for racial harmony cannot be quickly solved, governments must have time to deal with them successfully. By building trust, governments may buy the time they need.

The level of political trust existing at any time is the result of a complex process involving interactions with many variables. It is a changing reflection of a society's politically relevant conflicts and tensions. When trust is high, officials are in a better position to make commitments or adopt controversial policies aimed at the solution of difficult problems. Since high levels of trust are an important resource which cushions the impact of programs some groups find intensely objectionable, whenever substantial segments of the population begin to grow distrustful, it is important for the government to act before the level of trust drops to the point where the resources to solve societal problems and rebuild trust cannot be mustered. When distrust is growing leaders are faced with the delicate problem of making policies which reach the sources of dissatisfaction at a time when any action is likely to make some group angry—often even some of those among their traditional political constituencies. This is a high political price which leaders seek to avoid, thereby making the problem worse for themselves and often much worse for their successors. It is difficult to devise policies which can solve social problems, and still more difficult to build coalitions which can enact these policies and support their enforcement. The problem is doubly difficult when political trust is low.

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[64] E. Franklin Frazier, *Black Bourgeoisie* (Glencoe: Free Press, 1957).

[65] Gamson, *op. cit.*, p. 45.

In this section on the implications of our analysis for public policy we emphasize racial problems because they are the major concerns of our study and a fundamental problem faced by the nation, but we recognize that the legitimacy of our political system currently is being undermined by controversies over many other social problems. It is not possible to separate cleanly the racial crisis from the problems of poverty, the Vietnam War, the development of huge private and public bureaucracies, and the student rebellions. The government's response to all of them has contributed to political distrust. A thorough treatment of the problems of political disaffection would range far beyond the racial crisis, but our discussion is necessarily confined to these difficult issues by the limits of the data we have collected and the space available to us.

Effective public policies, which have the important side benefits of encouraging a growth in political trust, must be designed to meet the needs of the people they are meant to serve. As our analysis demonstrates, the factors influencing political trust among Detroit's population are not easily categorized. The causes of the suspicions and hatreds which have created the racial tensions we describe are deeply rooted in our history. No single, simple governmental program can be expected to eliminate the dangers we face or usher in a period of mutual compromise and understanding among blacks and whites. No matter what may be done, we are moving through an extremely difficult period of transition in race relations which almost inevitably will be accompanied by hurt feelings and racial conflict. The level of turmoil we experience, however, and the impact it has on the shape of American institutions, will depend in large part on the decisions made and the programs instituted by public officials. Even though the situation contains many intractable elements, if disaster is to be averted, the government must intervene whenever possible with effective efforts to build a truly inter-racial society.

Racial harmony cannot be achieved unless the damage done by the innumerable insults and racial slurs exchanged between whites and blacks during the last two centuries can be at least partially repaired. The desires of blacks for symbolic assurances of good faith, however, are becoming increasingly difficult to grant because of mounting white resentment. Any policy which seems to grant benefits to blacks is interpreted by segments of the white community as unfair, preferential treatment. Many of the whites in our sample believe that blacks are getting "something for nothing" simply because they are "trouble makers." During the last two decades many highly significant symbolic gestures have been made which were designed to signify that blacks were being included as full partners in American society. These gestures have fueled the fires of white resentment, helped to stimulate a major realignment in Southern politics, and prompted the rise of political candidates like George Wallace. Black aspirations have also risen during this period, so that gestures which once were appropriate or satisfactory are now denounced as "tokenism." This emotion-laden struggle over symbolic assurances threatens to destroy the government's credit within both racial communities at once, and it constitutes the central dilemma of domestic policy making in contemporary American politics.

We do not believe this dilemma can be avoided through a policy of prudent immobility. Although there is no easy way to reach a new state of racial accommodation, governments must make an effort to encourage that development. There are many ways in which governments may seek to halt the erosion of political trust caused by heightened racial tensions, but there is no policy, including inaction, which may be implemented without costs or dangers. Every governmental action will stimulate an intense reaction from some element within one of the two racial communities. Our analysis indicates, however, that if there is to be any hope of a resurgence of trust in the integrity of American governmental institutions, the promise that all public forms of racial discrimination will be eliminated must be fulfilled. As our findings demonstrate, personal experiences of racial discrimination are an important determinant of political distrust among lower status blacks. The persistence of discrimination against members of the black community is also one of the most important factors encouraging despair among upper status blacks. The constitution requires that racial discrimination be eliminated, and repeated declarations that it would be stopped have been made during the last two decades by public officials. Reducing efforts to fight discrimination at this time would only encourage disaffection and cynicism in both racial communities. It would lead citizens to view their government, rightly, as an unprincipled entity willing to retract its pledges of justice in response to threats. If our hypotheses about political trust are valid, this would lead even those who successfully made the threats to distrust government more than before.

Many courses of action are open to those wishing to attack the problems growing out of the contemporary racial crisis, but none will have a reasonable chance of success unless it involves a genuine effort at broad social reform. The developing sense of community among blacks in Detroit, to which we have referred so often, is founded on a common outrage about the material and social status of their group. Fundamental social changes have transformed the black community since World War Two and led to the widespread protests which marked the past decade. As a result of this controversy over the status and welfare of black Americans, an impressive new awareness of politics and public leaders has developed within the black community which is stimulating both higher expectations and greater impatience with the slow pace of change. The efforts of government to respond to black demands, no matter how small their scope or limited their success, have led to the creation of groups and the generation of similar demands for change and improvement within the white community. Many formerly quiescent ethnic and racial minorities and a significantly large segment of the white middle class have become critical of economic and social inequalities which were accepted as inevitable or simply ignored in the recent past. By responding to protest in the black community, the government has opened itself to similar demands from many other groups. It has also stimulated even higher expectations about its performance among all its citizens. Under these circumstances, once a process of competitive mobilization has begun, vigorous action to meet demands is necessary if the government hopes to maintain its integrity and the trust of its citizens.

Broad social reform does not come cheaply. Our analysis leads us to conclude that significant changes in the quality of public services and recognizable improvements in the level of well-being among large segments of the population are necessary if the society's problems are to be solved. In short, unless some means are found to raise large amounts of money for domestic programs, far beyond that now being used for these purposes, the problems arising from the racial crisis are almost certainly going to get worse.

When both the competence and good faith of the government are being called into question, action to deal with widely perceived social problems is urgently needed. The policies called for, however, must be designed to meet several important political criteria. Vitally needed money alone, even in unprecedentedly large amounts, will not necessarily insure the government's creditability. Democratic governments which wish to build trust must also convince the public that policies are the result of consultation and citizen participation, that the opinions of average people matter, and that individuals will receive a fair hearing from public officials. Plans for establishing neighborhood city halls, instituting a system of ombudsmen to aid citizens in making complaints against public officials, creating citizen review boards, electing advisory committes, and decentralizing large public agencies all come in response to the demand that rigid and insensitive public bureaucracies be eliminated. These proposals differ somewhat, but they all share the aim of improving the responsiveness of government to the complaints and desires of its citizens.

Plans for creating more responsive bureaucracies through various forms of administrative reorganization will have little chance of success unless they are accompanied by significant improvement in the benefits offered by government. The need for increased public expenditures and the need for a heightened sense of governmental responsiveness are closely linked. Programs which are designed merely to give the citizens a new assurance of the government's concern for their well-being without seeing that outputs are actually improved may produce better relations between citizens and officials in the short run, but in the long run they are likely to create even greater cynicism and disaffection.

Our analysis indicates that policies which do not result in increased benefits or better governmental services will be inadequate either to build political trust or to deal with the racial crisis. Our data also suggest that the agencies of government must become more responsive and sensitive to the needs and fears of both blacks and whites, and that individual citizens must be given more opportunity to influence bureaucracies whose actions affect their lives in fundamental ways. There are several policies now being proposed which might meet these criteria. One such option, which has recently received considerable publicity, is the decentralization of large city school districts into smaller units based essentially on established racial and ethnic communities. Advocates of this reform argue that it combines meaningful social change, through increased funds and improved services, with heightened administrative responsiveness, through a locally elected school board and increased consultation by teachers with members of the community. The plan is also meant to promote a new awareness and pride in cultural identity among blacks which will eventually lead to greater motivation for learning, an improved educational climate, and higher scholastic achievement. Plans for community control over important city services call for recognition of the fact that little progress has been made toward racial integration and that society has long tolerated the existence of many ethnic and racial enclaves within the cities. Decentralization is designed to capitalize on these social trends rather than combat them in order to promote racial pride and, ultimately, better education.

Decentralization of the administration of city services along racial lines may be initially satisfying to certain militant elements in the black community, and to many hostile, segregationist whites. By officially separating these antagonists and concentrating on enrichment of the existing communities decentralization plans are supposed to lay the foundation for a new racial harmony based on an even greater degree of cultural and racial pluralism than now exists. We believe, however, that there are serious dangers in any policy which grants legal recognition to the racial divisions within our society and employs them as official administrative categories.

The problems of resource allocation will likely be exacerbated once districts are clearly separated on racial and ethnic grounds. The temptation to deny a fair share of essential resources to minority groups will receive legitimation as each district of the newly decentralized system is inevitably called upon to provide the major sources of funds for its operations. This will only tend to encourage debilitating conflicts along racial and ethnic lines and likely cut down the funding available to the districts which need it most. The result will be programs which do not live up to heightened expectations and decreased political trust. The prospects will be increased for competitive mobilization of racial and ethnic groups and uncontrollable conflicts in the future.

We do not mean to discourage or belittle the importance of efforts to encourage a heightened sense of awareness and pride among blacks in their cultural identity. We do not believe, however, that the government should play a large role in this effort. Political leaders should concentrate, instead, on turning the society's attention away from an exclusive concern with racial conflict to a new broader concentration on problems of economic equality, the provision of medical care, housing, education and job training, the control of industrial planning and development, and the problems of environmental pollution. As coalitions of support are built on each side of these new controversies, racial conflicts might be displaced or reduced in importance. Schattschneider has observed that political "conflicts divide people and unite them at the same time, and the process of consolidation is as integral to conflict as the process of division."[66] Struggles over fundamental social issues, rather than pitting whites and blacks against each other on primarily racial grounds, might unite the two racial communities in pursuit of shared goals.

In other words, the best hope of obtaining an enduring racial peace and building political trust at the same time is through a massive, general attack on the outstanding social problems of the society which affect both whites and blacks. If these efforts at social reform are large and extensive enough, they would less likely be seen as special benefits designed solely for black people. Although there appears to be significant white opposition to spending more money to improve conditions in the ghetto and great ambivalence about the pace of change in the status of blacks, there is a sense that something must be done and a large reservoir of support probably exists among working and middle classs whites for extensive new expenditures on such things as housing, health and education which are aimed at improving the environment for all.

The value of truly large scale efforts to rebuild the cities, provide employment and job training, improve health care and meet other social problems is that through their magnitude and impressiveness alone, even if no special effort is made to emphasize their utility for black people, they might convince the black community that the government cares for their welfare and intends to provide them with the same benefits being provided for whites. Programs of this kind would provide material benefits greatly desired both by working class blacks and whites, and at the same time, might supply the symbolic assurances needed by the middle class blacks, without unduly antagonizing the whites. Edelman assures us that policies which distribute tangible rewards may also have significant symbolic overtones: "Because the requisite conditions are always present in some degree, every instance of policy formulation involves a 'mix' of symbolic effect and rational reflection of interests in resources, though one or the other may be dominant in any particular case."[67]

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[66] E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960), p. 64.

The public policies we advocate would not achieve racial harmony and widespread political trust in the short run—no policy can achieve that. The reforms we envisage would call for a large scale reallocation of resources which would certainly set off monumental political struggles, leading possibly to governmental stalemate. It may be impossible, given the rhetorical legacy of the past twenty years, to convince discontented whites that programs of general social reform are not merely further cynical efforts to pay off the black community. The time may have passed when broadly based interracial coalitions of support for social reform can be created. It also may be impossible to create responsive, efficient and flexible governmental agencies of the size needed to carry out the large national programs we have in mind.

There are no easy solutions to the current American racial crisis, especially in view of the questions being raised in so many quarters about the legitimacy of established governmental institutions and their ability to successfully meet the many demands being placed upon them. Our data reveal considerable political disaffection among blacks, but there is little desire as yet for separation from the political community as a whole. Blacks overwhelmingly support integration and even those who find violence acceptable still display a willingness to participate vigorously in conventional politics. Events and policies of the next few years may cause political distrust to be generalized to the point where people wish to separate themselves completely from the American political community, but this is not an inevitable development. A workable, interracial society will not evolve without positive, calculated efforts to create it, but if genuine programs of social change are aggressively pursued, citizens may slowly gain new trust in the government, a process which might spiral upward with repeated successes.

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[67] Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1964), pp. 41–42.

THE EFFECT OF THE AUSTRALIAN BALLOT REFORM ON SPLIT TICKET VOTING: 1876–1908*

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In the last two decades of political science, there has been considerable interest in the determinants of electoral behavior. Theories have been developed and tested on the sociological, psychological, and political antecedents of the vote. Virtually neglected in this search for determinants have been the institutional or structural properties of the electoral system itself. With a few notable exceptions,[1] such factors as electoral qualification requirements, registration laws, and ballot and voting systems have not generated much research enthusiasm. These institutional properties, however, provide the framework within which the effects of other independent variables must be judged. This applies to all basic electoral research—whether time specific or longitudinal—but especially to the latter. Too often longitudinal research tries to trace the causes of changing voting patterns without taking into account the institutional framework. A pointed example of this is Walter Dean Burnham's recent description of this country's "changing political universe" around the turn of the century[2]—a change which he ascribed to a breakdown in party competition and consequent voter alienation, but which undoubtedly could be partially, if not largely, explained by reference to the many institutional changes in voting rules which occurred during this period. The effects of institutional properties must be sorted out if the researcher is to establish reliable baselines against which to measure the effects of other variables.

The purpose of this study is to analyze the effects of one such institutional property of the electoral system—the Australian Ballot reform—on the changing split ticket voting patterns of the American electorate in the 1876–1908 time period. A theory is advanced to predict the ways in which ballot might be expected to affect split ticket voting. The theory encompasses not only the comparative effects of the Australian Ballot and the earlier unofficial "party strip" ballot, but also the differential effects of the varying internal formats the Australian Ballot assumed in the several states. A test of this theory is then made—involving a comparison of mean split ticket scores for the various ballot conditions across states within election years and within states across election years. Results show that the basic theory is confirmed. A test of Walter Dean Burnham's alternate theory of voting behavior for this time period is also made, and it is shown to be largely unsubstantiated insofar as it is dependent on the ticket-splitting phenomenon. Final interpretations are generalized to the role and influence of other institutional properties of the system on the voting decision.

I. THE AMERICAN BALLOT SYSTEM[3]

The Australian or "official" ballot was instituted in most states in the early 1890's. Massa-

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* This article is adapted from the author's doctoral dissertation of the same title (University of Michigan, 1968). Acknowledgments are due Angus Campbell, Warren E. Miller, Jack L. Walker, and Herbert F. Weisberg for their comments on the dissertation. A special debt of thanks is owed Philip E. Converse for reading and evaluating both the dissertation and this article.

[1] The few exceptions include Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), pp. 266–289; Stanley Kelley, Jr., Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," this REVIEW, 61 (June 1967), 359–379; V. O. Key, Jr., *American State Politics* (New York: Alfred A. Knopf, 1956); Donald R. Matthews and James W. Prothro, "Political Factors and Negro Voter Registration in the South," this REVIEW, 57 (June 1963), 355–367; and Warren E. Miller, "Memorandum to the President's Commission on Registration and Voting Participation" (unpublished Michigan Survey Research Center paper, 1963).

[2] See Walter Dean Burnham, "The Changing Shape of the American Political Universe," this REVIEW, 59 (March 1965), 7–28.

[3] The history of the American ballot system can be found in various sources such as Spencer D. Albright, *The American Ballot* (Washington, D.C.: American Council on Public Affairs, 1942); Eldon C. Evans, *A History of the Australian Ballot System in the United States* (Chicago: University of Chicago Press, 1917); Joseph P. Harris, *Election Administration in the United States* (Washington, D.C.: The Brookings Institution, 1934); Arthur C. Ludington, *American Ballot Laws: 1888–1910* (Albany: New York State Library, 1911); and J. H. Wigmore, *The Australian Ballot System as Embodied in the Legislation of Various Countries* (2nd

chusetts was the first to adopt it statewide in 1888[4] and, in less than eight years, approximately 90 percent of the states had followed suit. Rarely in the history of the United States has a reform movement spread so quickly and successfully. The example of Australia, which originated the Australian Ballot Law in 1856, and several other nations who adopted this law was sufficient to recommend the new system as a probable cure for the ills of the electoral administration of the time.

Before the introduction of the Australian Ballot, there was a separate ballot for each party called a "party strip" or "unofficial" ballot since it was prepared and distributed by the party instead of the government. Each party, in essence, made up its own ballot, listing only its candidates, and had "party hawkers" peddle it to the voters in what resembled an auctioneering atmosphere in and around the polling station. Each party also made its ballot distinctive by printing it in a different color and on a different sized sheet from those of the other parties. This assured instant recognition of the ballot by the voters, and, in turn, recognition by the party workers of which party's ballot a person picked up and voted. In addition, the fact that the actual act of voting was usually performed in the open further assured that the people had no right to a secret ballot.[5]

The Australian Ballot system changed all this. It was completely different from the earlier system in concept and orientation. The new ballot was state-prepared and state-administered—hence making it an "official" and uniform ballot which precluded the existence of party-prepared ballots. Secondly, the new ballot was a consolidated or "blanket" ballot, listing the candidates of all parties on it instead of only the candidates of one party. Last, the ballot was secret, an important complement to its being a consolidated ballot. The new system thus offered the voter an impartial, multiple-choice instrument, upon which he was allowed to deliberate and make a decision in the privacy of the polling booth. The intimidating party aura which so permeated the voting situation under the old system had been effectively dispelled.

Most states enacted ballot laws with the basic principles of the Australian Law—officiality, consolidation, and secrecy—intact. This still left them free to determine the internal format of the ballot, a matter on which there was not so much agreement among the states as there had been on the more basic provisions and general orientation of the Australian Law. Some states, following the lead of Massachusetts, looked once again to the original ballot law for cues and basically adopted its nonpartisan practice of aligning candidate names under office blocs. Some modification was made in this format, such as adding party names beside candidate names, but in general the ballot retained its objective, nonpartisan character. Other states followed the lead of Indiana in enacting a party column arrangement of candidate names on the official ballot. This format resembled a consolidation of the old party strips, placed side by side on the same sheet of paper. In essence, these states had overlaid their old party strip system onto the new Australian framework. Today, these two types of official ballot are known as the "Massachusetts office bloc" and "Indiana party column" arrangements.

The popularity of the two types of official ballot varied in this early time period. As Figure 1 shows, the office bloc format was the favorite of states in the beginning, but the party column design predominated after 1890, generally increasing in popularity over the years. The inference can be drawn that the early popularity of the office bloc form was due to its association with the original Australian Ballot Law. But this type of ballot, even when modified by the addition of party names, was too objective and nonpartisan to be in keeping with the style and heritage of American politics. Americans soon changed the format of the ballot to party column, to correspond more with the partisan nature of their political life. They perceived the important contribution of the Australian Law as lying in its basic provisions and not in its specific stipulation of internal format.

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ed.; Boston: Boston Book Co., 1889). In general, these references give much better accounts of the Australian Ballot era than the voting systems in use prior to that time. An exception to this is the book by Evans.

[4] Although the first Australian Ballots were adopted in 1888 and 1889, they did not become effective in federal elections until 1890.

[5] The party strip or unofficial ballot came into use in most states during the first half of the nineteenth century. Before that time, the states (or colonies) had other forms of paper balloting, voice voting, or such unorthodox procedures as letting corn or beans designate one's vote. At one time, in the early 1800's, several states allowed the voter to make up his own ballot. The parties, noting this, were motivated to print their own ballots in order to thwart the use of handwritten ballots. This practice of party-prepared ballots was upheld in a Massachusetts Supreme Court decision in 1829, setting a precedent which was not challenged until the beginning days of the Australian Ballot reform.

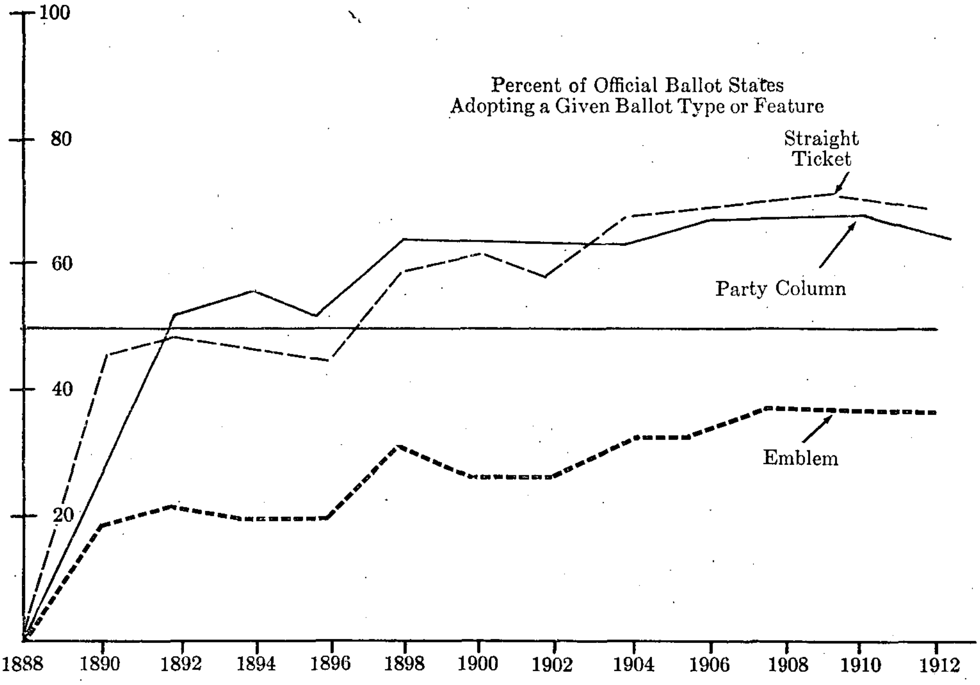

FIGURE 1. Trends in State Adoption of Partisan Ballot Types and Features

States also experimented in this period with adding certain features to the physical format of the ballot, such as straight ticket and party emblem devices. States would try a feature, evaluate its results, and then decide whether or not to keep it on the ballot. Figure 1 reveals certain trends in this experimentation and the resulting decisions made. For instance, the straight ticket addition seems to have been well received, since its popularity gained over the years. In part, such a movement is to be understood in the light of the increasing popularity of the party column form, the straight ticket option adding to the partisan flavor of this kind of ballot. This is not to say that such an option could not be or was not offered on office bloc ballots but rather that the general trend was for its incorporation onto the party column format. Party emblem was less used, but it also increased in popularity during this period, primarily, again, as a complement to the party column ballot.

II. BASIC THEORY

The theory proposed here centers on the effects on voting of two key dimensions of the ballot as an institution—its structural properties and the electoral environment in which it is used. Such information about the ballot is obtainable from state statute books and historical accounts of the time. Certain logical developments of this information and the use of past research findings about voting behavior help us to understand how ballot law and usage define the parameters and alternatives of the decision-making process at the polls and how the individual reacts under different ballot conditions. Two central hypotheses on ballot effects can be formulated. The first is that the Australian Ballot system had a stimulating effect on split ticket voting relative to its predecessor, the unofficial party strip ballot. The second is that the different internal formats of the official ballot themselves had differential effects on split ticket voting; in particular, we may hypothesize that the incidence of such voting was related to the degree of partisan orientation the particular ballot format displayed.[6]

Under the old party strip system, the voter's decision was, for all practical purposes, limited to a single choice—that of which party's ballot to cast. If the voter was interested in several candidates of different parties, he would have to weigh their relative importance and make a decision either to go with the one he liked best, or to go with what he considered to be the best overall party slate of candidates. The same type of decision model could apply to parties as well, if the voter preferred this alternative to that of ranking individual candidates. The important point is that the ballot system encouraged the voter to reduce his several choices to one and then take the party ballot corresponding to that decision.

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[6] These central hypotheses are based on a model of individual decision-making to be presented below. Testing such a model with aggregate election data is subject to the normal analytic cautions expressed by W. S. Robinson in his article, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review,* 15 (June 1950), 351–357. For a discussion of the Robinson argument of "ecological fallacy" as it applies to this analysis, see Rusk, *op. cit.,* especially pp. 67–70.

The party strip system did not deny the possibility of a split ticket vote being cast. There were ways to vote a split ticket if the individual had the motivation to find out how and to execute such a decision in an environment emphasizing partisan norms and allowing only an open vote. One method was to "scratch" a name off the ballot and write the name of another party's candidate above it. Another was to take two different party ballots, marking each for certain offices, and then have them attached together before depositing them in the ballot box. Certain variants of these voting schemes existed in other states. But it was rare to find instructions on how to vote "a split ticket" printed on the ballot or for a party hawker to mention such a possibility. The electoral environment discouraged deviant voting in most instances—the complete party control of the ballot encouraging and reinforcing a tendency toward straight ticket voting.

The voting situation was different under the Australian Ballot system. The voter no longer had to reduce his several choices to a single dominant choice; he could instead express his multiple preferences as they stood across the new consolidated ballot. If he needed guidance on how to do so, the ballot had voting instructions printed on it, and voting assistance was also obtainable from nonpartisan state officials. The presence of a secret vote gave further notice that the prevailing norms of the partisan world were capable of being broken at the polling station. In fact, the new system encouraged different norms more compatible with the changing milieu. Split ticket voting was on the rise because the opportunity clearly existed to be used. This did not mean that electorates immediately registered large split ticket tendencies overnight; voters had to adjust to the new ballot instrument and the new norms that the changing milieu created. Old habits had to be modified or broken to adjust to the new situation—habits that had been in existence for a long time.

Changes in voting habits (and the rate at which they took place) depended considerably on the internal format of the Australian Ballot. The two different formats of the official ballot could have effects of their own. For instance, the party column format, since it had consolidated the party strips of the earlier period, continued to suggest a more partisan vote than its counterpart, the office bloc ballot. The similarity in style of the Indiana format to its predecessor was compatible with previous voting habits, thereby making it more difficult to recognize and use the ticket-splitting potential of the new ballot. On the other hand, the Massachusetts ballot was conducive to an independent decision being made for each office. The alternatives it defined favored a multi-choice orientation rather than a single-choice one. The difference in orientation between it and the unofficial ballot probably prompted a faster adjustment to its split ticket capabilities than to those of the Indiana format.

The addition of straight ticket and party emblem features to the ballot could also have effects on voting behavior. Such partisan devices suggested some decrease in split ticket voting from the levels normally associated with ballot format alone, but the extent of this decrease depended on the ballot format on which they were displayed. The devices probably had only a small effect on the party column ballot since this format already provided a strong partisan orientation. But because these devices stood in greater contrast to the office bloc format, they were more salient to the voter and thereby considerably decreased the likelihood of split ticket voting on this ballot. In statistical terms, they must have had something akin to independent effects on the Massachusetts form, but only interactive effects on the Indiana ballot.[7]

III. OPERATIONAL DEFINITIONS

The first step in testing any theory is to operationalize its basic concepts; the second, to evaluate the merit of such operations from a methodological point of view. In this study, only the dependent variable—split ticket voting—needs to be operationalized. The independent variables of ballot types and forms are usable for analysis purposes directly as they are taken from state laws.

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[7] In contemporary research on survey data, Angus Campbell and Warren E. Miller show the voting effects of one of these partisan devices, the straight ticket provision. They find that there is a greater tendency toward party slate voting when a ballot format has a straight ticket device than when it does not. However, it is difficult to tell to what extent such effects are separate from ballot format considered as a variable since the correspondence between straight ticket provisions and party column formats (or multi-choice provisions and office bloc formats) is very close today. See Campbell and Miller, "The Motivational Basis of Straight and Split Ticket Voting," this REVIEW, 51 (June 1957), 293–312.

The research literature provides few guidelines for operationalizing the dependent variable. A measure authored by Walter Dean Burnham has gained some currency in the field and is related to one theoretical interpretation of the voting patterns of this period.[8] The decision was made to use the Burnham measure, primarily for two reasons—tests show it to be capable of meeting our major analysis requirements, and use of it will facilitate contrasting our findings with those of Burnham in a later section of this paper. Results of experimentation performed with the Burnham measure in order to understand its behavior and its methodological strengths and weaknesses will be given below.

The Burnham measure is defined as the difference between the highest and lowest percentages of the two party vote cast for the Democratic party among the array of statewide races in any given state and election year.[9] For example, state A in a certain election year has the following statewide races, with the results expressed as Democratic percentages of the total vote cast for the two major parties: President, 51%; Governor, 49%; Secretary of State, 48%; State Treasurer, 48%; and, Attorney General, 48%. The split ticket value for state A in this election year is 3%.

The central focus of the Burnham measure is on the competitive interaction of the two major parties in an electoral situation. This requires the exclusion of uncontested races and the virtual exclusion of races with third party activity from consideration in the split ticket computations.[10] Third party intrusions of a sizable nature would especially create additional variation in the two party vote which could not be considered a true split ticket effect for Burnham's or our purposes.[11]

As an aggregate-based measure, the Burnham index suffers from the same liabilities as other such measures. For example, it assumes that the "gross difference value" it obtains is due solely to ticket-splitting, yet one realizes that some portion of this value is attributable to other causes. Election fraud, accidental miscounting of election returns, etc., can register artificial effects in the index. Also, since the measure is an aggregate difference figure, it would not detect mutual crossovers between the two parties which have the effect of cancelling each other out.[12] After recognizing the weaknesses of an aggregate-based measure, the researcher must usually take the position that such problems are either minor or else distributed fairly randomly among the various cases in the data collection.

A fault more specific to the Burnham measure is that it does not provide for analysis of "comparable race sets" across states within a given election. The number and kinds of races from which it is computed can vary from state to state. The Burnham measure is rather best suited for analyzing ticket-splitting within states across election years as a means of controlling its "race composition factor."[13] A major part of the analysis in this paper uses the Burnham measure in this context. But an analysis across states is also an important interest. In this situation, use of the Burnham measure needs some intellectual justification since its values could fluctuate from state to state simply because of differences in the races composing it. Whether or not this is a disadvantage depends on how the index behaves as a function of the composition factor and what the relation of this behavior is to the *predictions* of ballot theory.

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[8] Burnham, *op. cit.*, p. 9.

[9] The definition of a "statewide race" was expanded in this paper to include an aggregation of the district congressional vote. As a check on this operation, another measure was constructed excluding the congressional vote, and, when compared, both revealed similar behaviors and interpretations in the subsequent ballot analysis.

[10] Burnham did not indicate what screening criteria he used to eliminate third party intrusions in his data. The rule established in this study was that no race would enter the computations if the third party vote contributed six or more percent to the total vote for that race. Admittedly, this figure was somewhat arbitrary and some might consider it too high; however, given the many and large third party movements in this period, the figure seems reasonable. To check this procedure, we also looked at those cases of states having third party vote figures greater than six percent for most of their races and yet where the percentage did not vary greatly across the set of contested races. These data, impure as they were, conformed to the same trendlines in the ballot analysis as the figures generated by the screening criterion selected for use with the Burnham measure. This gave increased confidence in the findings, and on two occasions in the ballot analysis below (Tables 5 and 8), we display the "impure" values of states with the "pure" values of other states, carefully distinguishing the two in such instances.

[11] For certain research purposes, the third party vote would be a useful statistic to keep in the split ticket picture. Third parties broaden the alternatives open to the voter and in this way contribute to split ticket voting in certain areas. However, when the concern is with fluctuations in the two party vote space, third party figures can only be viewed as creating needless disturbances in the data.

[12] Aggregate election analyses usually assume that voters crossover or split their tickets mainly in one direction—the direction of national trends. But even if this were not the case, the phenomenon of mutual crossovers would not contaminate our ballot analysis unless it occurred, for some reason, far more often in unofficial than in official ballot states, a proposition that seems unlikely.

Several tests of the Burnham index were conducted, primarily to determine the effect of the race composition factor, but also for the more general purpose of checking to see if the measure was indeed tapping ticket-splitting. With regard to the latter purpose, the measure was found to behave as one might expect, both statistically and theoretically. That is, as one increased the number of races in the index, its value would generally rise as a function of the probability of rare outcomes entering the calculations. As one included the presidential race with two or more state-specific races, its value also tended to increase in keeping with the theory that the presidential race is a more powerful generator of short-term influences in elections. While these two tests suggested the validity of the measure, they also highlighted the possibility that states contesting larger numbers or given kinds of races could have higher split ticket values simply because of their particular race composition situations. Hence, another test was called for—and this revealed that the race composition factor was not spuriously affecting our own predictions as to ballot effects. That is, it indicated that there were not larger numbers of races or given types of races associated with those ballot conditions predicted by ballot theory to have greater incidences of split ticket voting.[14] A last test also confirmed this lack of spurious correlation and gave further evidence of validity. Three measures devised to have "comparable race sets," and hence to control for the variable factor of race composition, behaved similarly and gave similar results in the ballot analysis as the Burnham measure.[15] The results from all the tests indicated that, assuming some care with the number of races per state, the Burnham index reflected split ticket voting adequately and avoided any noticeable confounding or contaminating effects in its operations.

IV. INTRODUCTION OF THE AUSTRALIAN BALLOT

The Burnham split ticket values, when computed for the 1876–1908 time span, are quite revealing. Figure 2 displays a trendline of rising values in the years in which the Australian reform took place.[16] However, until ballot has

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[13] By "race composition factor" is meant the types of races entering the Burnham index computations in any given state. Inspection of the data shows that this factor is fairly stable across elections though the index can in no way guarantee a complete control over this.

[14] While the primary purpose of this test was to examine the effects of the race composition factor on across-state, within-year analyses, it was also used in the within-state, across-year analysis context, once again revealing an absence of correlation between the race composition factor and the predictions of ballot theory.

[15] The three measures computed were based on the presidential-gubernatorial, presidential-congressional, and gubernatorial-congressional pairings of races. Ideally, they would have been the best measures to use in an analysis of ballot conditions across states within election years since they standardize race sets; however, there are several reasons why, in practice, they were not so useful. For one thing, they severely limit the number of cases entering the analysis. Even with the pairings mentioned above, one is assured of most states entering only the presidential-congressional computations since some of them do not contest the gubernatorial race in a presidential election year. With the addition of any third race to the index, more states would drop out of the computations for similar reasons. Second, the inclusion of but two races in an index for any given state does not allow much room for variation in the vote to occur. It certainly does not provide one with good estimates of a state's split ticket voting behavior. Third, such an index does not provide an adequate comparison with the results generated by the Burnham measure within states across election years. Comparability of the two modes of analysis is best achieved by using the same measure rather than two differently constructed ones. Also, it is more appropriate to use the Burnham measure consistently in order to compare our results with those produced by Burnham for his interpretation of the voting behavior of this period.

[16] Burnham, of course, also observed a rising split ticket trend in these years. His evidence for this was the split ticket scores of five states. See Burnham, *op. cit.*, pp. 13–14, 17–20.

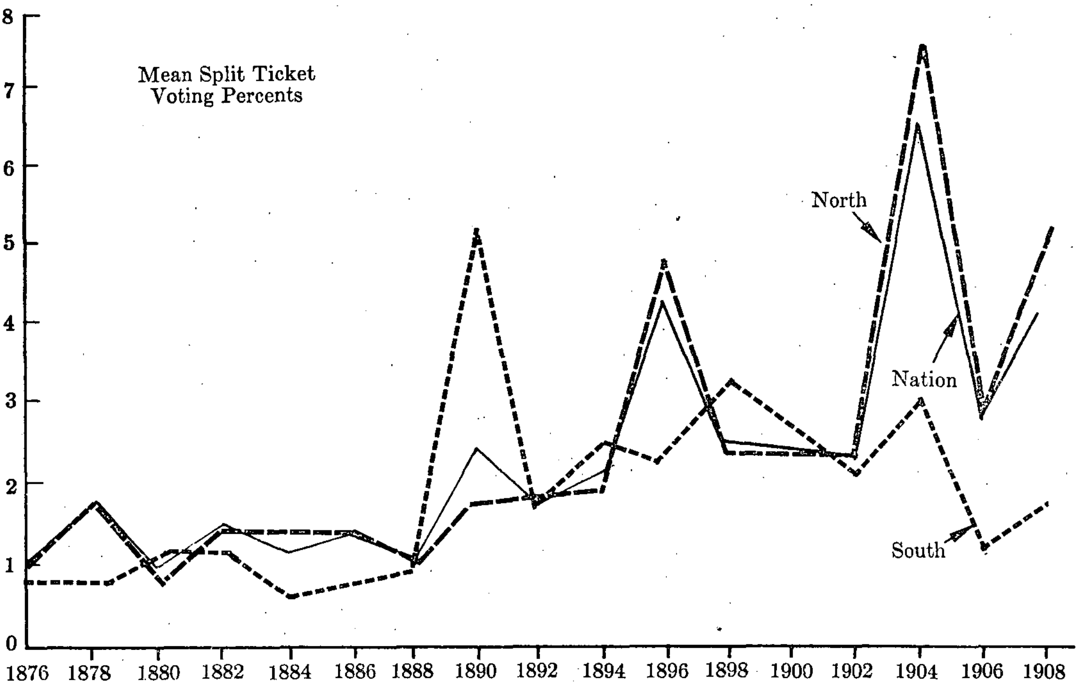

FIGURE 2. Means of Split Ticket Voting Index by Election Year for Nation and Region

been examined as an analysis variable, such a trend can only be regarded as indirect evidence of ballot's effect on voting. Perhaps there were other ticket-splitting stimuli that were particularly active in the Australian Ballot years. More direct tests of the central hypothesis need to be undertaken in order to determine the role ballot played in this split ticket voting picture.

One test is a within-year analysis of ballot conditions. Such a test also provides us with an election normalization device. Some control on different kinds and intensities of political stimuli can be put into effect by examining the unofficial and official ballot systems with regard to ticket-splitting in the same year. The results of this analysis are presented in Table 1.

A few points need to be made about Table 1 before it can be analyzed effectively. First, the number of cases in the table is necessarily limited due to (1) the ballot reform being accomplished in a short period of time, leaving few data points in the unofficial ballot categories, and (2) part of this general period, 1890–1908, being marked by fervid third party activity, making it necessary to drop some of the states (mainly in the Australian Ballot category) from the analysis. Second, the table contains not one but two unofficial ballot categories. Row (1) refers to the general type of unofficial ballot in use during this period—the "pure party strip" which has been described in previous sections. Row (2) includes all unofficial ballot states, combining the ballot cases of row (1) with the very few states (never more than five in the United States at any particular time) that used a deviant form of the unofficial ballot. This deviant form was called the "separate party strip system"—its distinguishing characteristic being that a separate party strip ballot was used for each office contested by the party instead of following the modal pattern of placing all offices contested by the party on a single party strip ballot. Row (3) of the table is, of course, the official ballot category.

The main finding in Table 1 follows the predictions of ballot theory. Regardless of whether one compares row (1) or (2) with row (3), the direction of mean values shows that the Australian Ballot states have more ticket-splitting than their unofficial counterparts in every election year in both the North and the nation.[17] The

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[17] In some election years, the split ticket distributions (as pictured on a standard low-to-high percentage value scale) were skewed to the right. Because of this, medians were computed to compare with the pattern of means. As one might guess, the results were similar in direction although some of the differences were smaller. Variance figures in this and other tables also behaved predictably in accordance with the pattern of mean values. Another way to look at this basic

TABLE 1. SPLIT TICKET VOTING MEANS OF UNOFFICIAL AND AUSTRALIAN BALLOT STATES BY ELECTION YEAR FOR NATION AND REGION[a]

Ballot Condition	Election Year										Overall Means 1890–1908
	1890	1892	1894	1896	1898	1900	1902	1904	1906	1908	
Nation											
Pure Party Strip (1)	1.5 (16)	0.8 (3)	— (0)	1.0 (1)	0.7 (1)	0.4 (2)	0.3 (2)	2.8 (2)	— (0)	3.3 (2)	1.5 (29)
All Party Strip (2)	2.5 (18)	1.0 (5)	0.6 (2)	4.0 (3)	0.7 (2)	1.4 (4)	0.5 (3)	2.6 (4)	0.1 (1)	4.3 (4)	2.2 (46)
Australian Ballot (3)	2.6 (7)	2.2 (13)	2.4 (11)	4.3 (34)	2.7 (26)	2.6 (36)	2.7 (20)	7.4 (21)	3.1 (13)	4.6 (21)	3.6 (202)
North											
Pure Party Strip (1)	1.7 (13)	0.8 (3)	— (0)	1.0 (1)	0.7 (1)	0.4 (2)	0.3 (2)	2.8 (2)	— (0)	3.3 (2)	1.5 (26)
All Party Strip (2)	1.6 (14)	0.8 (4)	0.9 (1)	1.7 (2)	0.7 (2)	1.6 (3)	0.5 (3)	2.7 (3)	0.1 (1)	4.8 (3)	1.7 (36)
Australian Ballot (3)	2.2 (6)	2.3 (11)	2.1 (9)	4.9 (28)	2.6 (24)	2.6 (27)	2.7 (19)	9.1 (15)	3.2 (12)	5.5 (16)	3.8 (167)
South											
Pure Party Strip (1)	0.4 (3)	— (0)	— (0)	— (0)	— (0)	— (0)	— (0)	— (0)	— (0)	— (0)	0.4 (3)
All Party Strip (2)	5.4 (4)	1.9 (1)	0.3 (1)	8.5 (1)	— (0)	0.9 (1)	— (0)	2.2 (1)	— (0)	2.6 (1)	3.8 (10)
Australian Ballot (3)	4.6 (1)	1.7 (2)	3.8 (2)	1.4 (6)	3.4 (2)	2.8 (9)	2.2 (1)	3.2 (6)	1.2 (1)	1.7 (5)	2.5 (35)

[a] Category (2) refers to all unofficial ballot states; category (1) refers to those unofficial ballot states which have all major statewide races on a single party strip ballot. Figures in parentheses denote the number of states on which the mean split ticket value was based for any given cell entry.

strength and consistency of this pattern of voting values attest to its own importance. The new official ballot allowed and encouraged split ticket voting. As a contrast, the South manifests a ragged, nonconsistent pattern. Partly this can be ascribed to inadequate cases for analysis purposes because of our data screening process. But also it is thought that the deviant form of unofficial ballot, which was used by a few southern states, motivated additional split ticket voting beyond that associated with the modal party strip ballot system. Such a tendency, given the few data cases in the table for the South, would accentuate the importance of this ballot in explaining the idiosyncrasies in this region's ticket-splitting picture.

A comparison of rows (1) and (2) by nation and region comments on the unique split ticket properties of the "separate party strip ballot system," the deviant form of unofficial ballot. For instance, in the nation, in all but one year the values of row (2) are greater than those of

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relationship is conveyed in Table 3 below, which presents the split ticket means for the ballot conditions for each state separately over all years, thereby holding constant the variation in the other states while viewing state X.

row (1). Since row (2) is an average of row (1) values and those of the deviant unofficial ballot states, one can easily infer that the deviant ballot is associated with the larger split ticket tendencies. The reason for this association lies in the fact that the voter had to choose a separate party ballot for each office for which he wished to vote; in so doing, he was faced with several possible decision-making situations, and unlike the voter in the modal party strip system, he did not have to reduce his several choices to a single dominant choice. The separate party strip system thus encouraged some modicum of ticket-splitting, at least generally greater than that evidenced in pure party strip ballot states. Its effects were more pronounced in the South than in the North, but the general tendency existed in both regions.

While the Australian Ballot and, to a lesser extent, the separate party strip system encouraged split ticket voting, a perusal of row (1) in the table reveals that the pure party strip states had very little split ticket voting in almost every election from 1890 through 1908. In fact, the values are similar in size to those registered in the same situation in earlier years before the Australian Ballot appeared in America. One might expect the values to rise in election years showing high split ticket values in the Australian Ballot states, but a comparison of rows (1) and (3) shows this rarely to be the case. For the separate party strip system, there seems to be some tendency in this direction as a result of its unique split ticket properties, but the modal party strip system presents a considerable obstacle to ticket-splitting in most elections, including some high ticket-splitting elections.

Another important aspect of Table 1 is the consistency of the unofficial-official ballot comparisons, given the varied stimuli probably associated with the different election years. The arrangement of the data is the same across all years—high and low stimulus, or turnout, elections, "Republican" and "Democratic" years. A more rigorous analysis of this across-year consistency in the data would be through regression techniques. Such techniques can pinpoint the amount of temporal covariation in voting properties between the Australian and unofficial ballot conditions. The resulting slope lines and coefficients would comment on the relative sensitivity within each ballot condition to whatever short-term forces lead to more or less ticket-splitting over the years. Such regressions were performed on the values in Table 1, considering the Australian Ballot values as the independent variable and the two unofficial ballot categories successively as dependent variables. The findings from both analyses revealed little variation in the effects of the unofficial ballot relative to the official one, indicating once again that people using the official ballot were more likely to translate whatever short-term forces there were in these years into greater split ticket voting. The official ballot provided the best opportunity for people to express their reactions to the short-term forces in the political environment.

A summary of the effects of the unofficial-official ballot conditions across all years can here serve to highlight the effects we have observed within given election years. As shown in Table 2, the progression of split ticket values is as predicted and reflects the need to consider the two pre-Australian forms as separate categories with separate effects. This logic fits well with the idea that the two are conceptually different and that this difference is shown to have predictable influences on ticket-splitting behavior. The dominant theme of the two is, of course, registered by the pure party strip system with its low frequency of split ticket voting, since it was the type of unofficial ballot used by about 90 percent of the states. The contrast between the two pre-Australian categories and the Australian Ballot reflects the split ticket potential existing with a secret, consolidated ballot. The opportunity is presented by the ballot and stands in contrast to the party emphasis in the earlier voting system.

Another way to look at the effect of ballot is to take each state separately and compare its split ticket voting before it adopted the official ballot with such voting after adoption. The analysis mode then shifts from comparing mean split ticket scores of the unofficial and official ballot conditions across states within election years to within states across election years. Within this mode, comparisons will usually be made for similar types of elections—keeping presidential and off-year elections distinct because of the differing stimuli associated with each.

TABLE 2. SPLIT TICKET VOTING MEANS OF UNOFFICIAL AND AUSTRALIAN BALLOT STATES OVER ALL ELECTION YEARS, 1876–1908[a]

Ballot Condition	Split Ticket Means
Pure Party Strip	1.2 (197)
Separate Party Strips	3.1 (20)
Australian Ballot	3.6 (202)

[a] The first two row categories refer to different types of the unofficial ballot.

TABLE 3. SPLIT TICKET VOTING MEANS OF UNOFFICIAL AND OFFICIAL BALLOT PERIODS OF STATES IN PRESIDENTIAL ELECTION YEARS, 1876–1908[a]

State	Unofficial Ballot Period	Official Ballot Period	Difference of Means	Category Cases
Alabama	1.10	—	—	1, 0
Arkansas	0.65	2.05	1.40	2, 2
California	1.12	2.15	1.03	4, 2
Colorado	2.62	3.88	1.26	4, 5
Connecticut	0.60	—	—	4, 0
Delaware	0.28	1.14	0.86	4, 5
Florida	0.70	2.70	2.00	3, 2
Georgia	—	—	—	0, 0
Idaho	—	2.03	—	0, 3
Illinois	1.18	1.20	0.02	4, 2
Indiana	0.38	1.10	0.72	4, 3
Iowa	0.93	3.40	2.47	3, 3
Kansas	2.77	0.72	−2.05	4, 4
Kentucky	0.45	1.58	1.13	4, 4
Louisiana	1.10	—	—	2, 0
Maine	—	—	—	0, 0
Maryland	0.48	1.54	1.06	4, 5
Massachusetts	1.15	6.58	5.43	2, 6
Michigan	1.00	6.62	5.62	3, 4
Minnesota	2.63	16.58	13.95	3, 4
Mississippi	2.00	—	—	2, 0
Missouri	1.57	2.00	0.43	6, 1
Montana	—	12.90	—	0, 3
Nebraska	2.05	3.65	1.60	4, 4
Nevada	1.80	4.35	2.55	4, 2
New Hampshire	0.72	4.08	3.36	4, 5
New Jersey	1.28	—	—	9, 0
New York	0.88	2.33	1.45	5, 3
North Carolina	0.48	—	—	4, 0
North Dakota	—	8.44	—	0, 5
Ohio	0.65	1.98	1.33	4, 4
Oregon	0.48	3.15	2.67	4, 2
Pennsylvania	0.42	0.70	0.28	4, 4
Rhode Island	0.55	5.46	4.91	4, 5
South Carolina	—	—	—	0, 0
South Dakota	—	1.40	—	0, 2
Tennessee	1.70	2.30	0.60	4, 4
Texas	0.55	3.40	2.85	2, 2
Utah	—	18.65	—	0, 2
Vermont	1.92	4.06	2.14	4, 5
Virginia	0.30	5.73	5.43	3, 3
Washington	—	3.23	—	0, 3
West Virginia	0.73	2.40	1.67	3, 5
Wisconsin	0.57	1.30	0.73	3, 2
Wyoming	—	4.06	—	0, 5
Means of State Means	1.1	4.1	2.3	128, 125

[a] The unofficial ballot category covers the period from 1876 through the last presidential election before change from the unofficial ballot for each state. The official ballot category covers the period from the first presidential election in which the Australian Ballot was used through the 1908 election. The designation "category cases" *refers to the number of elections* entering the split ticket mean computations for the unofficial and official ballot periods of any given state.

Practically speaking, the actual computations involve summing a state's split ticket values for elections of the same type, within each of the two ballot time periods, and then deriving a mean value for each period. Such an aggregation across elections within ballot periods has the advantage of sharpening the accounting of effects attributable to the ballot while dampening out (through mutual cancelling) the effects of any unique election year stimuli. The end product of such aggregating and averaging operations should be good estimates of each state's ticket-splitting behavior in the two ballot environments.[18]

The data for presidential elections are presented in Table 3 and resemble the other data sets with respect to the effects of the official ballot. All but one state—97 percent of the states in all—have a higher split ticket figure for their official ballot period than for their unofficial ballot period. In almost all of the states, the split ticket values of the two periods differ quite remarkably. Some states, such as Iowa, Massachusetts, Michigan, Oregon, and Rhode Island, reveal very little ticket-splitting in the party strip phase, but mushroom in value when the reform movement took hold. What is important in this regard is the increment in split ticket voting from the unofficial ballot condition (as expressed in ratios of ballot categories) and not a given percentage increase taken to be a standard for all states to pass or fail. The relative increase is the item of interest, and, in most cases, such an increase is quite large. A doubling, tripling, or quadrupling of such voting is commonplace among the various states.

Some excellent summary information can also be gleaned from Table 3. The import of its message is similar to earlier statements, but it can give us some indication of the average effect of the two ballot time periods. In the table, twenty-nine states qualify as having both unofficial and official ballot conditions. For these states, ticket-splitting goes from a mean value of 1.1 to 3.4 for the two ballot conditions, an increase of approximately three times. This is the average effect of the official ballot for states having both forms in presidential years. For all states, the figures deviate even more—1.1 to 4.1.

Several states, of course, were deleted from the first set of figures because of one or the other of two reasons—they used the unofficial ballot exclusively in this period, or, the converse, they adopted the official ballot, never having any previous ballot system. The incidence of ticket-splitting for the first category of states is very low—0.8 percent. The second category of

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[18] The voting estimates are "good" except for the fact that there is a rising split ticket curve in this period that makes sense substantively. Better estimates could be obtained by excluding the transition trends in the data.

TABLE 4. SPLIT TICKET VOTING MEANS OF UNOFFICIAL AND OFFICIAL BALLOT STATES IN SELECTED YEARS OF BALLOT CHANGE[a]

Ballot Condition	1890	1892	1894	1896	1898
Pure Party Strip (1)	1.5 (16)	0.8 (3)	— (0)	1.0 (1)	0.7 (1)
All Party Strip (2)	2.5 (18)	1.0 (5)	0.6 (2)	4.0 (3)	0.7 (2)
First Election Year of Official Ballot Use (3)	2.6 (7)	1.9 (9)	2.6 (1)	19.0 (2)	— (0)
Second Election Year of Official Ballot Use (4)	— (0)	3.0 (4)	2.4 (8)	2.0 (2)	2.8 (3)

[a] It should be noted that in order to trace the same set of states from the first to the second year of official ballot use, one must compare a given election in category (3) with the succeeding election year in category (4) of the data set. Some states, however, will be missing in one or both years of the data set because of failure to meet its screening requirements.

states, those western states entering the Union in the early 1890's or later, go in the opposite direction—they display the highest split ticket average in the study, 7.2 percent. Obviously, variables other than ballot were contributing to this outcome. The entrance of a territorial entity into national politics in itself must promote some reactions similar in kind to those shown for the ballot. The presence of the ballot makes it easier to implement such reactions. However, even when the large split ticket effects of these official ballot states are controlled for, the basic relationship between ballot and voting behavior still holds, as our example of the twenty-nine states above indicated.

The same story of ballot effects repeats itself in the off-year election figures so that little new information is to be gained from this data source. Approximately 85 percent of the states, in the off-year election context, show a rise in ticket-splitting in their Australian Ballot period. When off-year and presidential types of elections are merged, 91 percent of the states reveal the predicted direction.

Demonstration that ballot has an effect on split ticket voting is only one part of an analysis; for ballot theory to be substantiated, split ticket voting patterns must be shown to have changed in the states shortly after the new ballot was introduced. A reasonable prediction would be that, in accord with adjustment patterns, split ticket voting would show a rise in either the first or second election year in which a state used the new ballot. This proposition is first tested across states within these crucial years of ballot change. Table 4 presents mean split ticket values for three categories of states—unofficial ballot states (row 1 or 2), official ballot states in their first year of new ballot use, and official ballot states in their second year of use. According to the data, both the first and second years of ballot use seem to be good indicators of the effects of the official ballot. Sizable differences exist between the unofficial and official ballot states, as a comparison of either row (1) or row (2) with rows (3) and (4) indicates. States adopting the new ballot soon changed their voting patterns from those remaining steadfast to the existing system. Voters were seen to be adjusting to the new ballot and its ticket-splitting potential without much difficulty.

A second way to test this proposition of early ballot effects is within states across election years. In Table 5, split ticket values are listed for each state in its last election year using the unofficial ballot and the first two elections using the official ballot. Also listed are "difference values" between the last unofficial ballot election and the second election year in which the official ballot was used. (Use of the second, instead of the first, election year in computing these "differences" rests on the assumption that it is less disturbed by adjusting tendency "noises" of the electorate toward the new ballot.) The type of election, presidential or off-year, used in these unofficial-official ballot comparisons is decided separately for each state, according to which year its change of ballot occurred.

Of first interest in the data is the fact that approximately 80 percent of the states had greater split ticket scores after they adopted the new ballot. The difference values in column (4) show this, and the trend is repeated when one compares columns (1) and (2). Certainly, this finding would be more impressive if higher, yet its consistency with our earlier tables is further evidence of this type of patterning in the data.

The ballot picture is improved if one considers a few additional features of the data. For one thing, the deviations in the difference values are very small for the remaining 20 percent of the states. This is because these states adopted the party column type of official ballot, which in many respects resembled the earlier unofficial ballot situation. Of note here too is the observation that most of the party column states showing the predicted direction also registered but small difference values. (Asterisks denote the party column states in Table 5.) The next section will show that the party column form in the first few years revealed similar split ticket figures to the pre-Australian Ballot states. The habits formed by the party strip situation carried over to the party column form, with its arrangement of party strips and straight ticket

TABLE 5. SPLIT TICKET VALUES OF STATES IN LAST ELECTION BEFORE AND FIRST AND SECOND ELECTION YEARS OF OFFICIAL BALLOT USE, FOR COMPARABLE ELECTION TYPES[a]

State	Last Election Year Using Unofficial Ballot (1)	First Election Year Using Official Ballot (2)	Second Election Year Using Official Ballot (3)	Difference Values (Column 3 Minus Column 1) (4)
California	0.7	(2.7)	4.0	3.3
Colorado	1.6	1.0	5.6	4.0
Delaware*	0.1	0.1	2.1	2.0
Illinois*	0.9	(0.5)	1.4	0.5
Indiana*	0.4	0.7	(0.5)	(0.1)
Iowa*	0.6	(0.8)	6.0	5.4
Kentucky*	0.2	—	0.6	0.4
Louisiana*	1.9	—	0.6	−1.3
Massachusetts	1.2	3.6	2.2	1.0
Michigan*	0.6	(2.2)	0.6	0.0
Minnesota	3.8	(6.3)	(6.4)	(2.6)
Nebraska	2.6	(8.1)	3.2	0.6
Nevada	2.7	(7.2)	4.9	2.2
New Hampshire	1.0	1.4	6.8	5.8
New York*	0.5	2.3	1.6	1.1
Ohio*	0.0	0.7	0.2	0.2
Oregon	0.7	—	3.9	3.2
Pennsylvania*	0.5	0.1	0.3	−0.2
Rhode Island	(0.4)	2.8	3.2	(2.8)
Tennessee	1.1	4.6	1.9	0.8
Vermont*	1.6	2.1	4.3	2.7
West Virginia*	0.5	0.4	0.2	−0.3
Wisconsin*	(1.4)	1.4	(1.1)	(−0.3)
Means	1.1	2.4	2.7	1.6

[a] Most values in the table met the screening requirements of the Burnham index. Those enclosed in parentheses did not but were included as "impure" cases as defined in footnote 10 above. In order for a state to be included in the table, it had to have split ticket values in both the last election before and the second election of official ballot use—thus excluding those states that had no pre-Australian period, those never adopting the official ballot in the 1876–1908 time period, and those screened out of even the "impure data set" in these heavily "third party" election years. For a given state, the "difference value" compares the split ticket scores of similar types of elections (either presidential or off-year), depending on when the ballot change took place. The designation "—" entered for some states in column (2) indicates that these states' split ticket values were not included because of failure to meet the requirements of either the Burnham index or its "impure version."

* Denotes states having the party column format in their second election year of official ballot use.

provision facilitating the continuation of these habits.

Lending further weight to our ballot interpretation is the fact that with continued use of the party column format over time, the split ticket figures do increase. Admittedly, this tendency is not major, yet it does exist, as we shall see in Figure 3 below. The reasons for the increased split ticket use of the party column form over time are probably twofold—a continuing adjustment to the new ballot and a given election producing a desire to use the split ticket property of the ballot.

Thus, an analysis within states across election years substantiates the earlier results found by examining data across states within election years. The new ballot has effects on voting behavior not only, generally speaking, across time, but also fairly immediately after it is introduced in the several states.

V. TYPES OF AUSTRALIAN BALLOTS

The general conclusion of the last section was that the introduction and establishment of the official ballot facilitated a rise in split ticket voting. A more detailed question has to do with the relative contributions of the party column and office bloc types of this ballot to such ticket-splitting and also the contrast of their effects with those of the earlier unofficial ballot system.

Figure 3 presents the split ticket pictures for party column, office bloc, and unofficial ballot states in the North. The values in the figure are mean scores for each ballot condition within the election years of the 1890–1908 time period. Looking first at just the two types of official ballot states, one notes the wide divergence in their respective split ticket values, suggesting the influence of differing internal ballot formats on voting behavior. Ticket-splitting is significantly higher under the less partisan arrangement of the office bloc form than under the party column type.

It is also interesting to contrast each form of the official ballot with the unofficial ballot. The spread between the office bloc and unofficial ballots is sizable, reflecting a marked difference in the ticket-splitting potentialities of the two. Smaller differences are shown between the party column and unofficial ballots, especially before the turn of the century. The correspondence in orientation and design of the two ballots undoubtedly has much to do with their closer split ticket values. Both ballots have "party" as their main focus and both provide an easy means of voting a straight ticket, if in somewhat different senses.[19] Identification of party, its column or strip arrangement, and the attached emblem or color of ballot are conceptual parallels that

[19] There were two ways to vote a straight ticket in an unofficial ballot state—either deposit a given party strip in the ballot box unmarked or else mark it for all the candidates on it. Some states by law specified one way or the other, but some allowed both ways. In a party column state, one could vote a straight ticket by marking the entire ballot or by making a single mark if such an option was provided.

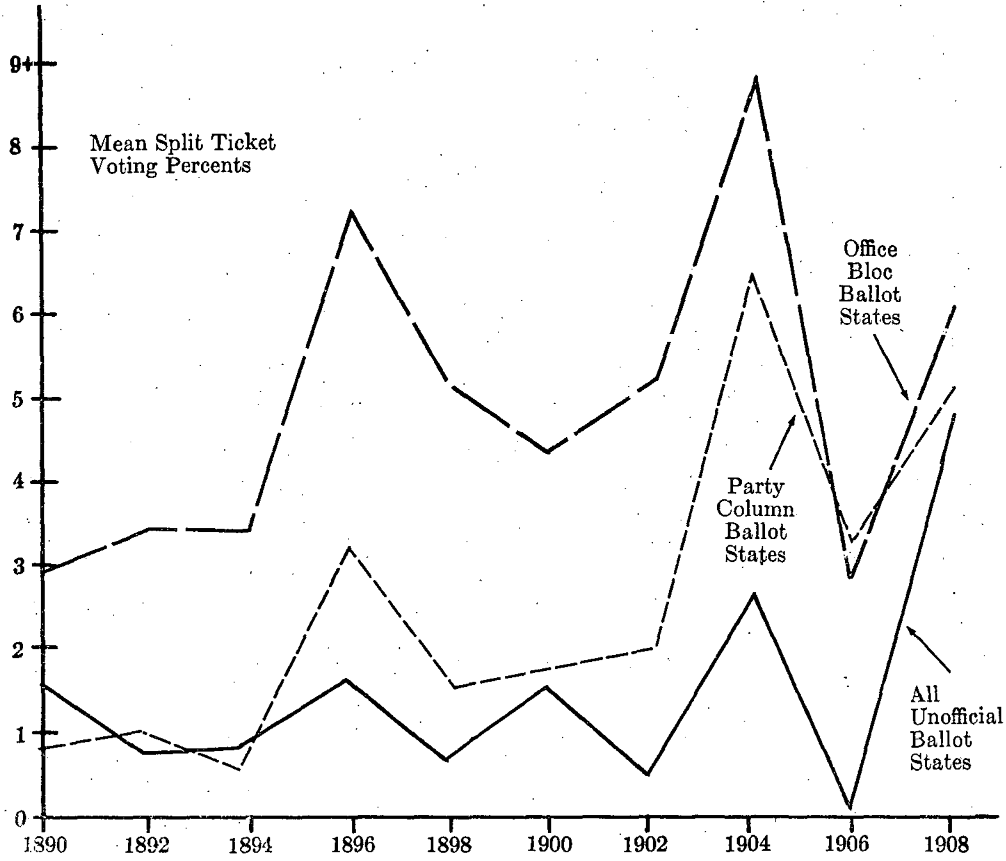

Figure 3. Split Ticket Means for Three Ballot Conditions of States, North Only

could facilitate the task of straight ticket voting for the elector.[20]

In spite of this stress on the relatively close voting patterns of the party column and unofficial ballots, it is important to note, on the other hand, the very existence of divergence. As predicted, the party column form of the official ballot had somewhat greater ticket-splitting than the unofficial ballot, since its consolidation and secrecy features provided a better opportunity for this type of voting.[21] Across time this difference in voting between the two ballots became more marked as electorates adjusted to the split ticket potential of the new ballot. It took time for voting habits to change, and the similarity of the party column and unofficial conditions was an important reason why such habits changed more slowly under the party column than under the office bloc format.

These comparisons of the three trendlines in Figure 3 can also give us a better understanding of the voting patterns earlier observed in Table 1. The large differences between the official and unofficial ballot conditions were, to a large extent, a product of the effects of the office bloc states—at least in the first part of the Australian period. It was not only the fact that a state switched to the official ballot that prompted a rise in ticket-splitting—it was also the condition of imposing a specific internal format on this ballot, namely the office bloc arrangement, that facilitated the larger increments in voting fluidity. After the turn of the century, the split ticket trendline of the party column states deviated more seriously from the unofficial ballot trendline—resulting in a greater contribution of the Indiana ballot to the sizable Australian Ballot ticket-splitting figures and, hence, to their differentiation from the unofficial ballot figures.

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[20] The split ticket differences between the party column and party strip systems become even smaller if one contrasts the latter with only those party column states having emblem and "single mark" features. The effects of these two devices will be explored in later tables.

[21] Two deviations occur in the data—for the years of 1890 and 1894. The deviation in 1890 is possibly due to the extremely small number of party column cases involved, 1890 being the first election in which this form of the Australian Ballot was used by any of the states. The deviation in 1894 is due to a separate party strip situation in one state. The differences between the two systems would have been larger and cleaner if one had contrasted pure party strip values with party column values; this tact was not taken in Figure 3 simply because two years had no values for the former condition.

A different analysis of the party column, office bloc, and unofficial ballots, involving within-state, across-year comparisons, produced the same results as shown in Figure 3. Office bloc states had higher mean split ticket scores than party column states, and both types of ballot states had greater mean split ticket scores in their official ballot period than in their unofficial ballot period. A study of those states that shifted ballot formats in their Australian years also revealed higher mean split ticket values for the period in which they used the office bloc format.

Within and across state comparisons have shown the impact of ballot format on voting habits. Obviously, this is a crucial variable in the overall ballot picture. But still there are other factors to consider—states could and did embellish these formats with straight ticket provisions and party emblems. Ballot formats did not have to exist in isolation, and the addition of these features could have added effects on electoral behavior.

The same sort of analysis was performed with these ballot features as with the party column—office bloc dichotomy in Figure 3. That is, states were divided into those having and those not having straight ticket or party emblem features on their ballots. The results were similar in trend to the party column—office bloc contrast, and, indeed, the differences in split ticket scores between the straight ticket and non-straight ticket situations were comparable in magnitude to those found between the party column and office bloc formats.

The finding of associations between voting patterns and the two ballot features is a step in the right direction—but only the first step. There is a further question as to how these features relate to one another and to the format of the ballot.

All possible combinations of the two partisan features and ballot format were surveyed to answer the question about their interrelationships.[22] They showed ballot format to be the dominant explanatory factor. The most revealing pairing of variables was that of ballot format and the presence or absence of a straight ticket provision. As shown in Table 6, both variables had considerable gross effects on voting, but larger differences occurred between types of ballot format when presence or absence of a straight ticket provision was controlled for than vice-versa.

TABLE 6. RELATION OF BALLOT FORM AND STRAIGHT TICKET PROVISION TO SPLIT TICKET VOTING, 1890–1908[a]

	Presence of Straight Ticket	Absence of Straight Ticket	Marginals
Party Column	2.7 (106)	3.0 (17)	2.7 (123)
Office Bloc	3.7 (16)	5.3 (63)	5.0 (79)
Marginals	2.8 (122)	4.8 (80)	3.6 (202)

[a] Cell entries are split ticket means computed over all election years, 1890–1908, for states having the ballot combinations listed in the table.

The importance of the straight ticket provision depended on the ballot format on which it was placed. In the case of the party column ballot, the split ticket value seemed little affected by whether or not a straight ticket option was present. The party column format in itself was facilitative of straight ticket voting, and the straight ticket provision added only the ease of marking just one "X" instead of marking an "X" for each candidate. It took little extra effort to mark "X's" down a column, especially given the earlier experience with the party strip system.

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[22] Originally, such combinations were viewed within election years and the direction of mean split ticket values was basically as predicted. However, one quickly confronted cells with few cases since several ballot configurations appeared with rare frequency in reality. Given these conditions, a better strategy was to aggregate ballot cases across time to better observe their effects and the following analysis is based on this procedure.

The straight ticket variable, however, took on more importance when on the office bloc ballot. Here it cut down the ticket-splitting potential of a ballot offering many independent decision-making possibilities. The voter could escape the problem of deciding on each successive office how he would vote by relying on the straight ticket option. Apparently, many voters preferred this alternative.

The results of the table suggest the presence of some interaction. It is possible that people were viewing the party column ballot with its straight ticket device as a whole, as an entity, with the underlying format defining this basic orientation, whereas this would seem less to be the case with the office bloc–straight ticket ballot. Interaction of device with format could be occurring in the former case, while in the latter, the device would be having more of a separate effect from that of format. Interaction of effects would most likely occur if format and device were conceptually compatible and less likely if they were antithetical in purpose and orientation.[23]

The other two pairings of variables provided us with additional information. Their results will be discussed here, but the actual data patterns are presented in a summary table below, Table 7.[24] The second pairing of variables, ballot format and party emblem, gave further evidence of the dominating explanatory power of ballot format. However, party emblem did have some influence in the party column case, making "party" even more salient to the voter and thereby decreasing split ticket voting tendencies slightly more than if it had not been present. Comparisons of the office bloc format with and without a party emblem could not be made since so few cases of the former pairing existed then and none do today. The problem of few cases made it difficult to hazard any test of interactive effects, although it seems plausible that people viewed the party column-emblem configuration as a single entity because of the compatible orientation.

The last pairing of variables, the two ballot provisions themselves, showed some modest decrease in split ticket voting when both partisan devices were present instead of just one. That is, the combination of a straight ticket and emblem provision reduced ticket-splitting in comparison to the "straight ticket only" type of ballot. The reason for this seems logical. The two devices share a common orientation and possibly interact in their effects, highlighting still further the partisan guideline. It is also of interest to note that the straight ticket—emblem combination, except for two cases, appeared only on the partisan-oriented Indiana format. The question of interactive effects between the two variables could not really be resolved since no state adopted a ballot form with an emblem but without a straight ticket.

The ballot combinations just presented can be summarized in one table. The cell cases decrease with an enlarged table, but the contrasts apparent in it are well worth the sacrifice. Table 7 presents these results.

Most of the discoveries of the pairwise comparisons mentioned above are repeated and highlighted here. The general influence of ballot format pervades the table. Some of the largest differences in the figures are attributable to it;

TABLE 7. RELATION OF PARTICULAR BALLOT CONFIGURATIONS TO SPLIT TICKET VOTING 1890–1908[a]

	Party Column	Office Bloc	Marginals
Straight Ticket and Emblem	2.3 (57)	3.5 (2)	2.3 (59)
Straight Ticket Without Emblem	3.1 (49)	3.7 (14)	3.2 (63)
Neither Straight Ticket Nor Emblem	3.0 (17)	5.3 (63)	4.8 (80)
Marginals	2.7 (123)	5.0 (79)	3.6 (202)

[a] Cell entries are split ticket means computed over all election years, 1890–1908, for states having the ballot combinations listed in the table. The ballot combination of emblem but no straight ticket did not exist for any office bloc or party column cases; for this reason, it was not included as a category in the table.

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[23] This interpretation of interactive effects in the party column—straight ticket case is suggested but not necessarily confirmed by the data at hand. As one can observe, the important off-diagonal cell entries in Table 6 have few cases and may not significantly depart from a value of 4.0, a condition indicating additive (separate) effects instead of interactive effects.

[24] A collapsing of some categories in Table 7 is needed to recover the points discussed for these two pairings of variables. However, this is a simple mental exercise and saves us from presenting two further bivariate tables at this time which would only convey the same information as Table 7.

and when the other variables are controlled, ballot format confronts one vividly as the dominant variable. The row marginals tell the story of decreasing split ticket voting with an increase in the number of partisan features in use. To understand this better, between-cell comparisons must be made. Reading the columns from bottom to top, one can see, in general, that adding a partisan provision to a given ballot type has the effect of decreasing the ticket-splitting. For instance, with the office bloc figures, ticket-splitting decreases by 1.6 percent when a straight ticket device is added. The counterpart party column cells show no decrease, this being the one deviant category in the data pattern. However, when going from straight ticket only to both straight ticket and emblem on the party column ballot, a decrease of 0.8 percent in split ticket voting is manifest.[25] When viewing the whole table from bottom right to top left, the decrease in split ticket voting progresses in a rather orderly and consistent fashion.

Thus ballot format, in its basic definition of the alternatives open to the voter, is a major key to understanding the incidence of ticket-splitting. The two partisan devices, straight ticket and emblem features, complement the party column format and, as a result, have some further role in heightening the salience of the "party vote" alternative. But clearly they are of secondary concern, interacting with the dominant theme portrayed by the ballot format. Such devices on an alien format, the Massachusetts type, stand in contrast to the voting alternative suggested by its structure. Something more akin to separate effects are registered in this case by the partisan devices.

VI. SUMMARY

The findings of this study of the Australian Ballot can be summarized as follows:

1) The introduction and establishment of the Australian Ballot in the states led to an increase in ticket-splitting in comparison to the previous ballot system. Placing both major parties on the same ballot and guaranteeing a secret vote were the major new provisions of the Australian Ballot that allowed and encouraged the expression of cross-party preferences in the polling booth. Government preparation and regulation of the ballot assured the enforcement of the consolidation and secrecy provisions and made the ballot uniform and impartial throughout a state.

2) The unofficial ballot system in use previous to that time was distinguished by very little split ticket voting. This lack of ticket-splitting was explained by the existence of non-secret, easily identifiable, separate party ballots and the way in which the party workers peddled them to the voters. The situation forced a single choice on the voter—which party's ballot to take. A deviant form of this ballot system, using a separate party ballot for each office, revealed somewhat higher split ticket patterns. The possibility of voting a split ticket was more apparent in this situation.

3) The extent to which ticket-splitting rose from the unofficial to the Australian Ballot depended on the type of internal format used on the Australian Ballot. The party column format displayed a slightly higher split ticket pattern than the unofficial ballot. The remarkable conceptual and physical similarity of the two explains the closeness of the difference—voters were familiar with a certain kind of ballot and the party column format was compatible with their earlier voting habits. Voters only gradually adjusted to the split ticket potential existing with this form of the Australian Ballot. The office bloc format, on the other hand, was a complete contrast to the unofficial ballot and therefore revealed much higher split ticket patterns than its predecessor. A more immediate adjustment to it was forced on the voter.

4) By implication from the discussion in (3), the party column and office bloc forms themselves had very different split ticket voting patterns. The party column ballot was associated with a low level of ticket-splitting, since it was the more partisan-oriented of the two. The office bloc ballot displayed a much stronger split ticket pattern as a result of its multiple-choice orientation.

5) The inclusion of a partisan feature on a ballot format also had consequences on voting patterns. A straight ticket addition on an office bloc ballot decreased split ticket voting because it worked against the multi-choice orientation of this format. On the party column ballot, use of a straight ticket alone had no effect, but when an emblem was also added, some decrease in split ticket voting occurred. Throughout, however, the dominant variable was the ballot format itself. When the partisan devices were included, they decreased ticket-splitting within a narrow range of the levels imposed by ballot format.

6) The general picture is one of ballot being an important explanatory variable in the 1876–1908 time period. While other theories can be offered to explain split ticket voting trends, the contributing factor of the ballot cannot be de-

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[25] Comparison of the counterpart office bloc figures, 3.7 and 3.5, is unwarranted since the latter figure is based on so few cases. Also, the cells with 17 and 14 cases must be interpreted with caution, as footnote 23 earlier suggested.

nied its role in any such discussion. It allowed an element of flexibility to enter the system and, perhaps, helped to shape new trends in party organization, new boundaries of political competition. The system was more in flux, and ballot had helped to set it in motion.

VII. BURNHAM'S THEORY

To date only one other theory has been advanced to explain the split ticket voting patterns of this period. The theory proposed by Walter Dean Burnham purports to explain the rise in ticket-splitting of this period by a breakdown in party organization and competition and a corresponding alienation of the voter from the political system. Before this breakdown, the electorate was, according to Burnham, quite firmly rooted in the party milieu and "party" served as the main reference group in the system.

Burnham's evidence for his theoretical conclusions is the split ticket records of five states, only four of which will concern us here.[26] But one could contend, given the above ballot analysis, that the increase in ticket-splitting observed by Burnham is, in fact, mainly due to the introduction and establishment of the official ballot in this period. The crucial question is how much variation in voting, if any, is there left to "explain" after ballot has explained all that it can. Any additional variation could be considered "fair game" for other variables, such as Burnham's, to interpret.

In order to answer this question, a series of "expected" split ticket values for the four states were computed—"expected" in the sense that they encompassed the mean effects on split ticket voting of all other states using the same Australian Ballot configuration; then, these figures were compared with the "actual" values of the four states.[27] The results are presented in Table 8 for each state's first election year of official ballot use, its second year of such use, and the remaining time spans for presidential and congressional election years, respectively.

The "difference values" in the table are the items to note. They sum up for the various years of interest the additional split ticket voting a state had left over after the effects of ballot were removed from the picture. Positive values indicate that some split ticket voting was left over; negative values indicate the reverse—that the states in question did not even achieve a split ticket value as high as the mean figure of other states using the same ballot configuration. A state-by-state analysis reveals that three of the four states had little or no additional split ticket voting for factors other than ballot to explain. The mean difference values for Ohio, New York, and Pennsylvania across these years are —.1, —.2, and .2, respectively. Only Michigan with a mean of 1.9 provides an exception to the general theme. The mean difference for all states over all years is 0.5, with Michigan providing most of the positive component in the computation. A value of zero would indicate that other explanatory factors were not needed for these states.

The reasons for such small difference values seem clear in the framework of previous ballot analyses. All four states had party column—straight ticket ballots and all save Pennsylvania had an emblem as well. Such states displayed only moderate split ticket increases in their Australian years relative to earlier ballot days. What increases in ticket-splitting there were to explain could easily be done by resort to the ballot variable alone. Only in Michigan did there seem to be other factors entering the picture.

The types of variables Burnham mentioned in his theory were supposed to be making widespread, not miniscule, changes in voting. While this could be the case in Michigan, it definitely is not so in the other three states. It is possible that a study of other states in this context might show additional ticket-splitting beyond

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[26] Burnham's fifth state, Oklahoma, is not relevant to this analysis since it entered the Union at the end of the time period covered in this study.

[27] The model used to compute these "expected" values is a simple additive one based on the formula, $Y = X + Z$. In the formula, X equals the state's pre-Australian split ticket effect, and Z refers to the net effect of other states with the same official ballot configuration as the state in question. The latter effect is computed by taking all such states for the election year (or year span) in question and subtracting their unofficial ballot mean from their official ballot mean. The type of election is held constant throughout the computations. An example for a state's first election year of official ballot use (year "A" in the table) will illustrate the procedure. The state of Ohio changed to a party column—straight ticket—emblem ballot in a presidential year. Therefore, its "X" value would be the mean split ticket value for presidential years in the unofficial ballot period. The "Z" value would find the same type of mean for all other states with that particular ballot configuration and subtract it from their mean value for the election year in which Ohio's ballot change occurred. Then, one merely adds the "X" and "Z" values together to get the "expected" split ticket value Y for Ohio in this particular election year.

TABLE 8. EXPECTED AND ACTUAL SPLIT TICKET VALUES IN SELECTED TIME PERIODS FOR FOUR STATES[a]

	Michigan				Ohio				New York				Pennsylvania			
	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D
Actual Split Ticket Values (1)	(2.2)	1.5	6.6	3.5	0.7	(0.2)	2.4	0.9	2.3	0.8	2.4	0.1	0.1	0.2	0.8	6.1
Expected Split Ticket Values (2)	1.2	1.0	2.1	2.1	1.0	0.1	2.3	1.1	1.2	0.7	3.3	1.1	0.1	1.1	3.0	2.2
Differences of (1) and (2) (3)	1.0	0.5	4.5	1.4	−0.3	0.1	0.1	−0.2	1.1	0.1	−0.9	−1.0	0.0	−0.9	−2.2	3.9

[a] Each state has actual and expected split ticket values for the following years: first election year of official ballot use, second election year of official ballot use, mean of the remaining presidential election years in the 1890–1908 time span, and mean of the remaining congressional election years in the 1890–1908 time span. These four time periods are designated in the table as "A," "B," "C," and "D," respectively. Those split ticket values enclosed in parentheses in row (1) are "impure" values as defined in footnote 10 above. The state of Pennsylvania was included as a data point in the official ballot period only until 1903 for three reasons. First, this state changed its ballot in 1903, providing a stimulus other states did not share—a stimulus that could contaminate the ensuing voter reactions in light of the previous experience with another official ballot format. Second, the time span for the second form (1904–1908) was too short to provide a reliable estimate of its split ticket effects. Third, the ballot change was to an office bloc—straight ticket combination, a virtual rarity among the states, thereby providing too few cases to ascertain its net effects in these other states.

what could be explained by ballot—but the crucial point is that the key examples of states used by Burnham do not confirm his theory. Even if other states showed such a tendency, Burnham would still have to account for other plausible features of this period which could cause ticket-splitting.

The ballot variable also seems to contribute to another of Burnham's findings. In his analysis, Burnham was concerned with not only the increased tendency toward ticket-splitting, but also toward "roll-off"—the tendency of the electorate to vote in greater numbers for high than for low prestige offices on the ballot. In the four states examined, Burnham found an increase in roll-off in the exact time period when the official ballot was adopted in these states. The official ballot contributed to roll-off since it was long and consolidated, requiring substantial effort on the part of the voter to mark all offices. But the unofficial ballot required less effort in voting than the official ballot—in many states, merely depositing the ticket in the ballot box, and, in some others, the marking of offices down a single column with no attendant decision-making involved as to partisan choices.[28]

VIII. THE INSTITUTIONAL ENVIRONMENT

The position of this paper has been that the Australian Ballot had an important effect on split ticket voting behavior. But its broader position has been that the institutional properties of the electoral system, considered either as an entity or as a network of component parts, have played and continue to play a crucial role in influencing and shaping voting behavior—in essentially defining the conditions and boundaries of decision-making at the polls. Stanley Kelley and cohorts, in their excellent article on registration variables, probably stated it best—it is a matter of "putting first things first."

By "institutional properties" are meant those laws, customs, and norms that define and regulate the broader entity known as the electoral system. Such things as registration requirements, electoral qualification laws, voting systems (e.g., plurality, proportional representation systems), ballot and voting machine arrangements, and the like are the framework of the system. Introduction of new properties or changes in existing ones affect the voting setting and, in turn, the behavior and attitudes of people. Yet, all too often the institutional environment has been taken for granted or as a "given" instead of being probed for its effects on political behavior.

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[28] In an interesting piece of research, Jack Walker has shown that roll-off also varies according to the type of official ballot in use—the office bloc ballot contributing more to a roll-off effect than the party column ballot. Walker also views his findings as a possible modifying factor on Burnham's theory as the following quotation suggests: "The argument made by both Burnham and Stiefbold that increasing roll-off is an indication of increasing political alienation at least is weakened by our finding that changes in ballot forms directly affect the amount of roll-off" (p. 462). See Jack L. Walker, "Ballot Forms and Voter Fatigue: An Analysis of the Office Bloc and Party Column Ballots," *Midwest Journal of Political Science,* 10 (November 1966), 448–463.

The attempt of this study and a few others has been to demonstrate the importance of these variables. In this study, important and often large effects on voting have been shown to be associated with one such institutional variable when it was first introduced into the environment. Comparing these effects to contemporary election data tells us that ballot had its greatest effects in the period of this study because of its novelty and fairly immediate impact, and then tapered off to a more normal, stable trendline as it became a commonplace in the political culture. Although the necessary research has yet to be done, the author would venture that the most crucial effects of any institutional property are visible in the early period of its use and then settle back to more normal differentiations in later years.

The explanatory power of institutional variables offers insight for future analyses. The introduction, establishment, or change in such factors can lend useful interpretations to shifts in electoral behavior. Take, for example, the changing vote turnout curve around the turn of the century, which cries for explanation. This sudden shift to lower turnout was another variable used by Burnham to justify his theory of party breakdown and voter alienation. But a more plausible explanation would be the effects of the newly enacted registration systems of the time, placing an additional barrier to the vote. Without much effort the reader can probably think of several other analytic uses to which this highly important set of variables could be put.

POLITICAL PARTIES, INTEREST REPRESENTATION AND ECONOMIC DEVELOPMENT IN POLAND*

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Discussing the character of the Polish party system elsewhere,[1] I have suggested a label of "hegemonic party system" for it, as well as for some other party systems based on similar principles. The hegemonic party systems stand midway between the mono-party systems and the dominant party systems as defined by Maurice Duverger.[2] In an earlier paper written jointly with Rajni Kothari we have suggested the following typology of party systems:[3]

1. Alternative party systems, where two or more political parties compete for political power with realistic chances of success;
2. Consensus party systems, where multi-partism does exist but one political party commands in a lasting way the loyalties of a predominant majority of the citizens and permanently runs the government;
3. Hegemonic party systems, where all the existing parties form a lasting coalition within which one of them is accepted as the leading force of the coalition;
4. Mono-party systems;
5. Suspended party systems, where political parties exist but are prevented from regulating political life by other forces (for instance, by the military);
6. Non-party systems, where the government is ideologically hostile toward the political parties as such and does not permit them to function.

Quite obviously, this typology does not exclude mixed types of party systems. On the contrary, the very fact that in political life nothing is absolutely permanent leads to the emergence of transitory types of party systems. However, I am satisfied that the above-mentioned typology better reflects the differences and similarities of the party systems which exist nowadays than the traditional numerical typology of multi-, two-, and one-party systems.[4]

Every party system is a product of history. It is also functionally interrelated with the social, economic and political characteristics of the society in which it operates. The present paper is an attempt to interpret the Polish hegemonic party system from the point of view of its historical roots, its role in the process of interest representation and the interrelation between the economic development of the country and its party system.

I. HISTORY: THE EMERGENCE AND EVOLUTION OF THE PARTY SYSTEM

The hegemonic party system in Poland is based on the lasting alliance between the three political parties and non-party political and social organizations. These three are:

a) The Polish United Workers' Party. Founded in December 1948 through the unification of the Polish Workers' Party and the Polish

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* The present paper has been written during the author's stay in the University of Manchester as Simon Visiting Professor, October-November 1969. I should like to express my deepest gratitude to the Simon Fund and to the Department of Government, University of Manchester, for giving me this excellent opportunity to write it.

[1] Cf. Jerzy J. Wiatr, "One Party System: The Concept and Issue for Comparative Studies," in Erik Allardt and Yrjö Littunen (eds.), *Cleavages, Ideologies and Party Systems* (Helsinki: Transactions of the Westermarck Society, 1964); and, Jerzy J. Wiatr, "The Hegemonic Party System in Poland," in Jerzy J. Wiatr (ed.), *Studies in the Polish Political System* (Wroclaw; Ossolineum, 1967). The latter has been reprinted in Erik Allardt and Stein Rokkan (eds.), *Studies in Political Sociology* (New York: The Free Press, forthcoming).

[2] Maurice Duverger, "Sociologie des partis politiques," in Georges Gurvith (ed.), *Traité de Sociologie*, vol. 2 (Paris, 1960), p. 44.

[3] Rajni Kothari and Jerzy J. Wiatr, *Party Systems and Political Pluralism: Comparisons between India and Poland,* paper presented at the VIth World Congress of Sociology, Evian, France, 1966, mimeographed.

[4] Our typology differs in details from that proposed by Giovanni Sartori in his paper on *Typologies of Party Systems—A Critique,* presented at the VIIth World Congress of Political Science, Brussels 1967 (mimeographed). Sartori distinguishes between the following types of party systems: 1) one-party, 2) hegemonic party, 3) dominant party, 4) two-party, 5) moderate pluralism, 6) extreme pluralism, 7) atomized. His (4), (5) and (6) types should certainly be taken care of by sub-dividing our (1) type, while our (5) and (6) are subtypes of his (1). Otherwise, the two approaches are very similar.

Socialist Party, the membership of the P.U.W.P. reached 2 million in the last two years. The party controls the most important positions in the government and has a clear majority of about 51% in all parliaments elected in last twenty years. In our terminology it is the hegemonic party of the system.

b) The United Peasant Party, founded in November 1949 through the union of the two political parties of the peasantry which had existed previously. With a total membership of about 450,000 and about 25% of seats in the Diet it is the second strongest party in Poland.

c) The Democratic Party, founded nominally in 1939 and more strongly expanded after 1945; its total membership amounts to about 75,000 and it holds about 8.5% of the seats in the Diet.

The three-party alliance is a permanent coalition based on common participation in the left-wing underground during the Nazi occupation of the country. In January 1942 the various Communist and radical political organizations merged in the new "Polish Workers' Party", which, after having failed to persuade the anti-Communist underground that the time was ripe for the comprehensive policy of national unity in the struggle against Hitler's Germany, launched a political offensive for a front of national unity led by itself. On New Year's night of 1944 the National Council of Homeland was established as a provisional parliament of the nation. It was composed of the representatives of the Polish Workers' Party, left-wing socialists, left-wing activists of the peasant movement (the so-called "People's Will" group) and the radical democrats, as well as by the representatives of the left-wing underground armed forces. In July 1944, when the first part of the Polish territory had been liberated by the Soviet Army, the Council created the Polish Committee of National Liberation, which at the end of the year transformed itself into the Provisional Government of the Polish Republic. After the end of the war and on the basis of the Yalta and Moscow agreements between the United States, the Soviet Union and Britain, the new, enlarged government was formed and received diplomatic recognition from the Western powers (the provisional government had been recognized by the Soviet Union previously).

The liberation of the whole country (January 1945), the expansion of leftist political parties and, finally, the legalization of two centrist parties, which joined the coalition after the Yalta and Moscow conferences, resulted in the emergence of six political parties. Four of them (the Polish Workers' Party, Polish Socialist Party, Peasant Party and Democratic Party) were the continuation of political forces which had created the National Council of Homeland during the war. They considered themselves permanent allies and in 1946 founded the Bloc of Democratic Parties for contesting jointly the forthcoming parliamentary election (January 1947). The centrists who joined the provisional government in June 1945 organized themselves in two political parties: the Polish Peasant Party and the Labour Party. The first became a strong political force, while the latter (based on former membership of the pre-war Christian Democracy) never managed to achieve any prominence.

To be sure, the six parties did not represent the whole spectrum of politics. The right-wing parties of the emigration as well as some traditional center parties (including the right-wing socialist party) refused to recognize the new political situation of the country and launched an armed attack against the new government through the network of their underground organizations. In 1945–1947 a state of semi-civil war prevailed in most of the country with casualties of tens of thousands on each side. During the electoral campaign the right-wing underground forces supported the Polish Peasant Party against the Bloc of Democratic Parties. As the result of this the election itself became a part of the civil war and led to a direct show of force.

After the Democratic Bloc's victory in the election the two centrist parties tried to play the role of parliamentary opposition during most of 1947. By the end of the year, however, splits in their ranks, defection to the West of some of their leaders and the pacification of the country by the victory of the government forces over the underground resulted in de facto defeat and liquidation of the anti-Communist opposition in all its forms. The two peasant parties merged in 1949, and the Labour Party dissolved itself in 1950, some of its members joining the Democratic Party. Therefore, the end of the "struggle for power" stage of the Polish post-war history was marked by the consolidation of all legally existing political parties within the permanent coalition and by the consolidation of political power in the hands of this coalition. The war and post-war history of the country explains the roots and characteristics of the party system in the sense that:

(1) it allows us to trace back the origins of the cooperation between the Workers' (Communist) party and the two other leftist parties to the years of the struggle against Nazis during the war;

(2) it explains how the logic of revolutionary seizure of power eliminated anti-Communist forces from the political life of the country;

(3) it explains the hegemonic role of the

Workers' party in the system in terms of the role it played during the critical years when the future of the nation was being decided.

The realization of the new political order resulted in the transformation of the war-time and revolutionary-time coalition into a permanent "National Front" alliance which, under the leadership of the P.U.W.P., runs the country at the beginning of the second quarter-century period of its post-war history. Within this alliance the hegemonic role of the P.U.W.P. is manifest in its five main functions:

(a) The party represents and expresses the socialist ideology underlying the entire political system. It determines the fundamental aims and values which constitute the basis for the functioning of the political and socio-political institutions of the country.

(b) Through the activity of its members on the institutions of state and social organizations, the party harmonizes the functioning of these institutions with the basic goals of the system.

(c) The party determines the general directives of policy-making by the state institutions.

(d) The party mobilizes a number of citizens to participate in political decision-making at various levels of government.

(e) The party recruits and educates cadres of political leaders operating within the party as well as in the institutions of the state.[5]

The two other parties of the alliance play a different role within the system. Their main functions can be best described as follows:

(a) They particpate in political decision-making at various levels of government and in the practical execution of these decisions.

(b) They constitute additional, although not the only, political representation of the social strata within which they operate (peasants in one case, white collar workers and craftsmen on the other). It has to be kept in mind, however, that the Workers' party operates among these social strata as well; the peasant membership of the P.U.W.P. is about equal to that of the U.P.P.

(c) They constitute forms of political participation not based on clear ideological commitment to Marxism-Leninism "'They are not," to quote Wladyslaw Gomulka, "Marxist parties, but they are independent parties of socialist democracy and of actualization of the current tasks of the joint policy of their members and sympathizers."[6]

In decision-making and decision-executing, in interest representation and in enlarging the possibilities of political participation, the non-Communist parties play an important role. They are no longer considered "transmission belts" of the Workers' party,[7] but its valuable and independent allies within the whole structure of the political system.

II. INTEREST REPRESENTATION AND POLITICAL PARTIES

Since no modern society is absolutely free from differences of interests, the way in which interests are represented within a system of government constitutes a very important characteristic of any system. But let us first address ourselves to the question, what interests do exist within the Polish society?

We can distinguish between two meanings given to the term "interest" in the current literature. In the first, interest is the policy objective which is favorable to a group of citizens and either less favorable or unfavorable to other groups. In this sense, we say that the increase of prices paid for agricultural products is in the interest of farmers; and—as far as this interest is concerned—we may treat farmers as an interest group. In the second sense, "interest" is a policy objective which is being pursued by a more or less clearly defined sub-group of citizens. The abolition of capital punishment may be a declared policy-objective of an "interest group." However, using this term does not mean that the members of the group *campaign* for something more favorable to them than to the others; obviously it is not the case in our example.

Interest representation includes representation of interests in both these senses of the term. In politics it can, moreover, count less what interest is being represented than how it is represented and to what extent the interest representation is a part of the functioning of the policy. In this respect one has to take into account an important precondition of the socialist state: the declared willingness to exclude from the political process the interest of those social classes whose very existence is incompatible with the long-term policy objectives of the socialist transformation of the society. This restricts the interest representation to those interests which are considered to be in harmony with the principles of the system.

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[5] Cf. Wiatr, "The Hegemonic Party System in Poland," *op. cit.*

[6] Wladyslaw Gomulka, *O neszej partii* (On our Party) (Warszawa, 1968), p. 437.

[7] The application of this term to the non-Communist parties in the early nineteen-fifties reflected a tendency to reduce their role within the system. The trend was, however, reversed after October 1956. Cf. Z. Mikolajczyk and E. Patryn, *Struktura i funkje partii chlopskiej na przykladzie ZSL* (Structure and functions of a peasant party: the case of UPP) (Warszawa, 1968); and A. Lopatka, *Kierownicza rola partii komunistycznej w stosunku do panstwa socjalistycznego* (Directive role of the communist party in relation to the socialist state), (Poznan, 1963).

Within these restrictions we may enumerate the following types of interest groups:

(1) economic interest groups, such as trade unions, peasant associations (in Poland, Peasant Self-Aid Union, farmers' circles), associations of private (small) business;

(2) professional interest groups, such as the writers' union, General Technical Organization, etc.;

(3) interest groups representing various segments of society such as the Socialist Youth Union, Rural Youth Union, Women's League, and the veterans' organization—Union of Fighters for Freedom and Democracy;

(4) religious organizations, particularly religious socio-political associations of Roman Catholics (Pax, Znak etc.); the Catholic Church, albeit not an association, may itself be considered a powerful interest group of a special status and structure;

(5) national organizations, such as Ukrainian, Byelorussian, Lithuanian, German and Jewish socio-cultural societies;

(6) regional associations, such as an association for the development of Nowy Sacz county, in which prominent people born or educated in this county work in the interests of their mother-region.

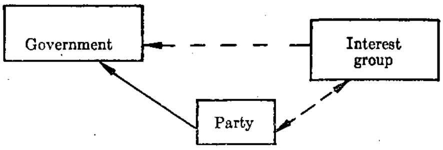
The interest representation takes, therefore, two forms. First, within the political parties sectional interests are represented—on the basis, however, of the accepted principle that no party should serve a narrow interest only, but that all parties should seek means of reconciling the various interests with the all-national interest of the country. In this respect, there is a difference between the Polish United Workers' Party and two other parties. The P.U.W.P. is committed to the policy of combining all-national interests (interpreted also as the prospective, historical destiny of the working class) with the current interests of the manual workers, peasants, white collar workers and professionals, all of them being strongly represented among the party membership and in its leading organs. The U.P.P. and the D.P., on the other hand, are committed to combining all-national interests with the special interests of the peasants and craftsmen, respectively. Dyzma Galaj, a prominent theoretician of the U.P.P. and well-known sociologist, defines the peasant character of the U.P.P. by pointing to the following functions of this party: 1) it accepts responsibility for the correct development of economic and "socio-cultural relations in the countryside"; 2) in the process of socialist education of the citizens it "takes into account first of all agricultural, mostly peasant population"; 3) it defends peasant interests in direct contacts with agricultural institutions, which show certain "bureaucratic characteristics"; and 4) it "looks for those forces among the peasants which can accelerate and deepen the processes of socialist education of the society."[8] The representation of group interests is, therefore, considered to be a two-fold process. It includes influencing the represented segment of the society in the direction favorable to the general policy of the ruling alliance, as well as defending the interests of this segment in direct contacts with the state authorities.

As far as interest groups in their relations to the political parties are concerned, two characteristics of the system are of special relevance:

(1) The political parties, and particularly the P.U.W.P., influence most of the interest groups through the activity of party members within those groups. Except the religious groups (and to some extent also some of the national groups) party members tend to play an important role in the leadership of the interest groups.

(2) The political parties (again particularly the P.U.W.P.) determine the general line of the activities of the state apparatus and, through delegating their members to the government service, see to it that the administration puts into practice the general line as it has been defined by the political parties.

From these two traits of the system a kind of triangular relationship among the government, political parties and interest group emerges:



If we take as an example the trade unions, we can say that within the limits of the general objectives of the economic policy they are concerned with: (a) mobilizing the workers to achieve the goals of that policy, and (b) representing their interests in the consultations leading to the defintion of policy objectives and economic strategy, as well as during the actualization of this policy.[9] The state apparatus is charged with the task of policy execution; therefore between it and the trade-unions differences of views (or emphasis) can and do emerge. On the factory level workers' self-management is an instrument of reconciling differences of this type.

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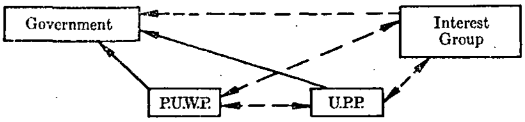
[8] D. Galaj, Introduction to Z. Mikolajczyk and E. Patryn, *op. cit.*, pp. 14–18.

[9] This point has been discussed at length by Krzysztof Ostrowski, "Rola zwiazkow zawodowych

The above model could be extended and made more complicated in the case of some other types of interest groups. If we take the peasantry, we have the relationships of the following type:



However, a general characteristic of interest representation vis-a-vis the state is that the process is in most cases indirect (religious groups being the only exception), the political parties playing the role of moderator among the various interests which have to be represented.

Undoubtedly, this is a model. Its usefulness lies in the fact that we may start with the model and look for the situation where, for one reason or another, the process of interest representation does not work properly. This, however, is beyond the subject of this paper. What I want to emphasize is the crucial role of the party system in interest representation and the fact that although the function of interest representation has a universal importance for modern societies, the political forms of this representation differ considerably.

III. POLITICAL PARTIES AND ECONOMIC DEVELOPMENT

Post-war Poland is a country whose public policy has been focused on achieving rapid economic development within the life period of one generation. Official ideology and public statements put the emphasis on mobilization of all available resources to make the rapid economic progress possible. During the twenty-five years of her third independence Poland has advanced from the group of backward, predominantly agricultural countries to that of medium industrialized ones. The total industrial output in 1966 was 545.5% and the industrial output per capita 427.3% of that in 1950. In 1966 the inhabitants of cities and towns represented 50% of the population, the comparable index for 1950 amounting to 39% and that for 1931 to a mere 27.2%.

Economic development being the principal focus of official policy, it is interesting to find to what extent the political mobilization through the political parties constitutes a factor in the economic change. Our major hypothesis, which we have tested in empirical research conducted during recent years, has been that *political mobilization is a precondition of economic development* in the socio-political system of contemporary Poland. The extent to which this hypothesis can be generalized to cover other countries (for instance, other socialist countries or some of the developing countries) depends in part on the availability of empirical material for the countries concerned.

One of the steps to test the hypothesis was the analysis of aggregate data from 50 Polish counties (poviats) conducted in the Political Sociology Department, Institute of Philosophy and Sociology, Polish Academy of Sciences.[10] On the basis of correlation and factor analysis of 25 indices of economic development a synthetic index of economic development has been worked out. Every county has then been given a score on the scale of economic development. Finally, correlation coefficients for economic development and five indices of political mobilization have been computed. The results are shown in the matrix in Table 1.

Two conclusions emerge from the matrix in Table 1.

(1) political mobilization, measured as a ratio of political parties' or political organizations' membership in the population, positively correlates with economic development;

(2) various forms of political mobilization are mutually complementary rather than alternative (since they also correlate positively).

Before we discuss the actual character of the first interrelationship, a few additional findings related to the second one should be mentioned. An independently conducted analysis of aggregate data on economic development and political mobilization in the Polish countryside, summarized in Table 2, shows again that political mo-

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w polskim systemie politycznym" (Role of trade-unions in the Polish political system), unpublished Ph.D. dissertation, Institute of Philosophy and Sociology, Polish Academy of Sciences, Warsaw, 1968.

[10] Cf. Jerzy J. Wiatr, "Przemysl i instytucje polityczne Polski Ludowej" (Industry and political institutions of People's Poland) in: Jan Szczepanski (ed.), *Przemysl i spoleczenstwo w Polsce Ludowej* (Industry and Society in People's Poland) (Wroclaw: Ossolineum, 1969). For the statistical analysis of the data on mobilization and economic development I should like to express my gratitude to Dr. Krzysztof Ostrowski.

TABLE 1. POLITICAL MOBILIZATION AND ECONOMIC DEVELOPMENT

Political Mobilization Indices	Country Economic Development Scores				
	2	3	4	5	6
1. Members of P.U.W.P. per 10,000 population	.67	.39	.50	.47	.37
2. Members of U.P.P. per 10,000 rural population		.13	.70	.58	.27
3. Members of S.Y.U. per 10,000 population			.18	.33	.60
4. Members of R.Y.U. per 10,000 population				.39	.18
5. Members of U.F.F.D. per 10,000 population					.29

PUWP—Polish United Workers Party; UPP—United Peasant Party; SYU—Socialist Youth Union, RYU—Rural Youth Union; UFFD—Union of Fighters for Freedom and Democracy, veterans' organization.

TABLE 2. POLITICAL MOBILIZATION AND ECONOMIC DEVELOPMENT IN THE COUNTRYSIDE

Indices of Political Mobilization	Development Scores	
	2	3
1. Members of P.U.W.P. in the countryside as a ratio of labor force employed in agriculture	.90	.82
2. Members of U.P.P. as a ratio of labor force employed in agriculture		.75
3. Members of R.Y.U. as a ratio of labor force employed in agriculture		

bilization through the two principal parties and the R.Y.U. correlates positively.[11]

In still another study conducted in 342 villages, Stefan Dziabala has established positive correlations between the strength of the P.U.W.P. in these villages and the level of economic and cultural development of the villages, as well as between the strength of the P.U.W.P. and that of the UP.P. and the R.Y.U.[12]

One can, therefore, generalize that similar social conditions are favorable to political mobilization within the P.U.W.P. and within the other parties and political organization. Political mobilization can, therefore, be regarded as one dimension, regardless of the party or organization through which it is channeled. This in itself seems to be an important finding, since it emphasizes the "nonantagonistic" character of the political pluralism within the Polish system.

Turning now back to the interrelation between political mobilization and economic development I should like to concern myself with the nature of the interrelation. Is economic development a condition favorable to the growth of political mobilization? To test both hypotheses an analysis of the time dimension is required. This has been provided for by the study of aggregate data relating to the growth of the P.U.W.P. between 1964 and 1968 (the dates of the IVth and Vth Party Congresses respectively).[13]

In this analysis level of economic development (for 1967) has been distinguished from the rate of economic development (1963–1967), allowing us, therefore, to account for the time dimension in the study of interrelation is between economic growth and political mobilization. As the result of this the following pattern emerges:

(1) The level of economic development positively correlates with the ratio of P.U.W.P. members among the adult population.

(2) The rate of economic development positively correlates with the ratio of P.U.W.P. members among the labor force employed in the nationalized economy.

(3) It negatively correlates both with the level of economic development (the backward regions having been developed more rapidly due to the public policy of reallocation of resources) and with the ratio of P.U.W.P. members among the adult population.

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[11] Krzysztof Ostrowski and Adam Przeworski, "A Preliminary Inquiry into the Nature of Social Change: the Case of the Polish Countryside," in Jerzy J. Wiatr (ed.), *Studies in the Polish Political System, op. cit.,* p. 86.

[12] Stefan Dziabala, "Dynamika i struktura rozwojowa wiejskich organizacji partyjnych" (Dynamics and developmental structure of the rural party organizations), *Zycie Partii,* no. 10, 1962, p. 10.

[13] Cf. Krzysztof Ostrowski and Zbigniew Sufin, "Problemy rozwoju partii miedzy IV and V zjazdem" (Problems of the growth of the party between IV and V Congresses), *Nowe Drogi,* no. 1 (236), 1969. The findings are based on correlation, factor and regression analyses. Data have been collected on the level of voivodships (provinces). The analysis has been done in the Institute of Philosophy and Sociology, Polish Academy of Sciences.

To put it in a different way, at the earlier stages of industrial expansion party membership increases rapidly among those employed in the nationalized (mostly industrial) sector of the economy. On the latter stages of economic development, it spreads to the other sectors of the population, particularly to the rural one. Tentatively, since the findings at our disposal have to be enriched by future inquiries, I should modify the original hypothesis in the following way: *political mobilization within the key sector of the economy is a precondition of economic development, while political mobilization of **a** larger sector of the public tends to be a consequence rather than a condition of economic development.*

IV. CONCLUSION

The empirical studies of political parties in Poland are still in their early stage. The analyses I have referred to in the preceding section do not exhaust all the types of problems under study in this field. For instance, the personality characteristics and motivations of the party members are being studied extensively with an emphasis on the influence of political activeness on value orientations and professional performance of party members.[14]

The character of party systems and of political parties themselves in the socialist countries of Eastern Europe is different from that which prevails in the West. It needs to be studied not as a departure from the "natural" state of things, as seems to be an approach adopted in some area studies, but as a socio-political reality of different character and determined by different types of social conditions. Empirical research on this subject, still in its early stage, does not yet allow us to undertake broader comparative analyses. It might, perhaps, be regarded as a step in this direction.

[14] Positive interrelation between political activeness and education and/or professional performance of party members has been emphasized in the studies conducted by Z. Mikolajczyk and E. Patryn, *op. cit.*, as well as by A. Stasiuk, "Czynni i bierni czlonkowie partii w zakladzie przemyslowym" (Active vs. passive party members in an industrial enterprise), *Studia Socjologiczno Polityczne,* no. 16, 1964. All the studies on this subject are case studies and do not permit statistical analysis on a larger scale.

COMMUNICATIONS

To the Editor:

As a British observer of the American political scene, it was heartening to learn that professional scholars are so willing to use their scientific expertise to shoulder moral and political responsibilities. Particularly enlightening were the results of the Columbia University survey showing that 66% of the sample said let's get out within eighteen months, while 43% were confident that this would not lead to "large-scale slaughter of the South Vietnamese" (perhaps they were comforted by the thought of small-scale slaughter). Here indeed is moral responsibility of startling proportions. Evidence of comparable political responsibility is found in the revelation that only 35% believed that a viable South Vietnamese government would emerge after withdrawal. One is reminded of the (modified) Shakespearean injunction: Let us lay down our arms against a sea of troubles and by abdicating end them.

Bryan S. R. Green

York University, Canada

To the Editor:

The urge to write to the editor of the *Review* comes upon me in two-year cycles; thus it is likely to be the last letter I will be writing you, at least in your capacity as an editor.

This is not really a Dear-Sir-You-Cur-type of letter but it comes mighty close to being one. What set me off was the headnote for Professor Caspary's article in the June issue, p. 536, especially the second paragraph. I gather there is a long waiting list for articles to be published in the *Review* and beyond that list there must be quite a number of political scientists who do not consider their work to be sterile exercises and who would value the opportunity to present their work to their colleagues in the premier review of the profession. Given all this, why did you find it necessary to fill the Review with 12 pages of a sterile exercise four years old, a work the author himself considers to be infinitely less significant than his present enterprise?

I have not asked Professor Caspary why he chose to permit publication of a "sterile exercise" because I do not think that it is any of my business to ask him such questions. But I feel that as a member of the Association whose dues support the publication of the *Review* I am entitled to ask why my membership dues are used to print a "sterile exercise."

With best wishes for a peaceful existence as an editor emeritus,

Indiana University

Alfred Diamant

(Editor's note: ever in keeping with the spirit of our times, the referees and the editor concluded that the progeny should be judged on its merits, not on the progenitor's low opinion of his own work.)

To the Editor:

Steven Brown and John Ellithorp's article on emotional experience in political groups is an interesting blend of psychology and political science, but it demonstrates the pitfalls involved. Psychological analyses of political phenomena are customarily viewed with a suspicion that they really constitute a below-the-belt attack on behalf of the authors' political predilections. One combats ideology, not with critique, but with the discovery of non-rational motivational factors.

Specifically, the authors regard liking Senator McCarthy or agreeing with his low-key tactics as indicators of dependence, an infantile longing for a charismatic leader; while disagreeing with McCarthy's tactics or regarding him as a poor loser is evidence of emotional detachment.

It's rather curious: these rational counterdependent types who represent Bion's work group, who reject hostility and are goal-oriented score +5 when it comes to disagreeing with McCarthy, expressing strong views and contributing their two-cents worth (4, 15). Rather than having a high work orientation, they would appear just as likely to be carping malcontents who wanted all things their own way.

By the same token, a refusal to get involved in time-wasting petty arguments is regarded, not as steadfast goal-orientation, but is evidence of withdrawal from threatening or competitive situations (4, 28). And when factor 1 makes an ambivalent score on an ambivalent question, it is regarded as "failing to come to grips with what is implied" (29—What is implied? that defeated McCarthyites should have chosen between Humphrey, Nixon and Wallace? or that they might feel all was finished once McCarthy had lost?).

If we wish to play psychological games, we might note that the nature of our presidential campaign system is precisely that of dependence upon a leader, that once a leader is chosen it is customary to acknowledge his right to conduct the type of campaign that befits his own style, and that the obsession with the way in which McCarthy conducted his campaign represents a need to rationalize failure. The good political worker limits his concern with such matters and goes out and rings doorbells.

Since the authors argue for the importance of subjectivity in a theory of political action, it is only fair to agree with them and to contend that they strongly disagreed with Sen. McCarthy's tactics and felt that the McCarthyites should have rallied behind Hubert Humphrey, and that they are engaged in a hatchet job on all those whose responses differed.

May I suggest that a psychological analysis of political behavior demands categories that carry no opprobrium, rather than an attempt, conscious or unconscious, to divide into devils and angels or healthy and sick.

WILLIAM A. BAKER
Loop College, City Colleges of Chicago

TO THE EDITOR:

We appreciate Professor Baker's comments, but wish to point out that our paper was on a theory of groups and not on the McCarthy movement per se. We admired Eugene McCarthy and, of course, did have feelings about the issues, but the fact that Baker thought we were defending the factor 4 position—whereas factors 2 and 5 represent the views with which the authors, respectively, would identify—leads us to think we succeeded somewhat in keeping our biases to ourselves.

Professor Baker is correct in labeling our efforts a game; for political analysis, like chess, has its rules. For example, in offering an alternative explanation to our own, Baker notes that refusal by factor 1 respondents to engage in petty arguments (observation X) *could* reflect goal-orientation (Baker's alternative theory) rather than dependency (Bion's theory). The posing of alternative explanations is quite legitimate under the rules of the game. However, if one is to play the game properly he must try to explain all and not simply those observations which seem to fit. For example, the factor scores also indicate factor 1 eschewed group-related activities (observation Y) and felt personally ineffectual (Z). Do Y and Z follow from Baker's theory? We think not. However, Bion's theory accounts for X, Y, and Z and so appears to be a better theory.

We anticipate that much of Professor Baker's disappointment stems from a suspicion (unfounded, we assure him) that we were trying to do in the McCarthy people. Unlike our critic, perhaps, we do not equate "irrational" with "nonrational," nor either of these with "bad." The democratic personality may automatically and unthinkingly (i.e., nonrationally) defend threatened liberties. Such behavior need not be rational to deserve our support.

We are quite certain that the McCarthy people were among the most articulate in 1968. The rationales they articulated, however, are the subject matter of the political-theory game, whereas analysis of why they articulated what they did is part of the political-psychology game. Both games complement one another and are legitimate under the rules of political inquiry. We sought to study the psychological aspect, and went out of our way to say so, and ought not therefore be scored for not attending to the former. One can do just so much in a single study.

STEVEN R. BROWN
JOHN D. ELLITHORP
Kent State University

TO THE EDITOR:

In undertaking a reconsideraton of Fascism, a subject charged with so much negative emotion, I fully expected to encounter a certain measure of hostility and perhaps incivility. Moreover, in an informal discipline such as ours—characterized as it is by but few common standards for evaluation—I fully expected variable assessments of my book, *The Ideology of Fascism.* My expectations have been largely fulfilled. The book met with considerable hostility, some incivility, and widely variable assessment. The conjunction of emotion with variable (if not idiosyncratic) standards, has produced evaluations which vary from "an admirable study of Fascist thought" to be "recommended"—"a fresh, clear, intelligent, intelligible and carefully researched book"—"perhaps the most important and provocative analysis of Italian Fascism since Nolte" —"a book argued at a high level of ability" (*Journal of Politics, Il Borghese* [Rome], *Choice, The Economist* [London])—to that of a book that "lacks perspective" afflicted with "lapses from ordinary scholarly accuracy" producing "few fruitful results" (*Annals of the American Academy*). Sometimes my scholarship is characterized as "thorough" and "precise"—and sometimes as "tedious" (*Washington Post*). My style is sometimes conceived as "intelligible" and "clear"—and at other times as "bearing a close resemblance to sludge, with a precisely articulated crystalline structure" (*Washington Post*).

Recognizing the likelihood of just such variable assessments, I expected that professional reviewers would be circumspect in their judgment. Where their judgment was all-but-unremittingly negative, I fully expected arguments to accompany it. In this expectation I have been sadly disappointed by Mr. Dante Germino's review (*APSR,* June, 1970).

I am not particularly concerned about Mr. Germino's judgments regarding my literary style. These judgements tend to reflect largely personal preferences. Whatever substantive discussion such judgments could possibly generate exceed the prescribed limits of this reply. I am more concerned about Mr. Germino's evident indisposition to provide arguments in support of his broad, negative substantive judgements. Instead of arguments he entertains us with special punctuation—exclamation points, underlinings for emphasis—innuendo via the employment of *sic*—as well as the exploitation of metaphor (I "mechanically" discover things, and I "play a game"). Such strategies are appropriate where the propositions in question are self-evidently absurd. That the propositions advanced in my book do not so qualify is evidenced by the fact that the section of my book to which Mr. Germino takes special exception—the treatment of "leftist" modernizing movements as sharing genus and species traits with Mussolini's Fascism —has been found by other reviewers to be not only "welcome," but "convincingly demonstrative"—that it is "carefully worked out and deserves close attention" (*Journal of Politics and the Economist*). Even the critical reviewer for the *Times Literary Supplement* found my theses "challenging and challengeable—and well argued." If Mr. Germino finds these theses so "sweeping and questionable as seriously to detract from the value of the book," it seems legitimate to suggest that he provide *arguments* to support his claim. I'm sure that there exists a sub-culture in which "Wow!" and "Right on!" constitute the counterfeit of arguments, but I hardly think such curious strategies can pass for evidential support for judgments in a professional journal.

For example, I am sure that Mr. Germino knows that others have identified the same similarities subtending Fascism and Leninism (particularly in the form it took under Stalin), as I have. Franz Borkenau, Peter Drucker and Ely Halevy, among others, early argued the case. Trotsky found "deadly similarities" between Stalinism and Fascism—as did Bruno Rizzi (*La lezione dello Stalinismo* [Rome: Opere nuove, 1962]). Carl Friedrich and Z. Brzezinski argued similarities at the genus level. More recently, in an article in *Survey,* Brzezinski suggested that "the highest stage of communism is fascism"—which seems to argue identification at the species level. Ludovico Garruccio (*L'industrializzazione tra nazionalismo e rivoluzion* [Bologna: Mulino, 1969]), in turn, has made a case for genus and species similarities between Fascism, Leninism, Maoism and "Third World" one-party states. Angelo Del Boca and Mario Giovana provide a journalistic argument in support of a thesis of "fascism" in African one-party states (*Fascism Today* [New York: Pantheon, 1969]). Surely, if the theses were so "sweeping and questionable" as Mr. Germino affirms, they could be defeated with summary argument. Mr. Germino could defeat a significant number of analysts at one fell swoop. For some reason known only to himself Mr. Germino has sought our defeat not with argument, but with special punctuation.

The interpretation of the Fascist phenomenon, as well as its contemporary "relevance," are serious topics. I invite Mr. Germino to publicly exchange *arguments* with me. I am sure we can find an appropriate vehicle for such an enterprise.

There is more to Mr. Germino's review than a questionable intellectual strategy. He apparently objects to the view (which I hold) that one of the principal tasks of political science is to generate descriptive, predictive and explanatory propositions. Political science is, of course, equally obliged to evaluate. But if the former without the latter is sterile, the latter without the former is puerile. I'm sure that Mr. Germino does not suggest that the analysis of Fascism (or fascism) be restricted to accounts that "unequivocally reject fascism" and preoccupy themselves with the "deep scars" identified with fascist experiments. We are all familiar with the horrors of Stalin's purges, with Mao's massacres and with Castro's "revolutionary justice"—but I suspect that any political historian who dwelt on those aspects of those regimes would be charged with "cold war hysteria," and perhaps "anti-communist paranoia," which fault understanding, objective assessment and adequate classification. There is no dearth of literature devoted to the horrors of fascist regimes. There are few, however, that attempt any reasonably objective account of Fascist ideology, the public *rationale* of Fascism. My clear purpose was to provide that rationale for inspection. That rationale, placed against fascist practice (something I have urged readers to do), affords a more substantial comprehension than any catalogue of enormities.

Mr. Germino evidently has a special methodology which permits us to avoid something he calls "pseudo-objective descriptivism." His methodological convictions are apparently the ultimate basis for his criticisms. Again, I suggest that these are important issues that cannot be resolved by his pronouncements. I invite Mr. Germino to a public discussion of them.

Finally, perhaps because of Mr. Germino's special epistemological insights, he seems driven to the judgment that when I argue that Fascism, Leninism and Maoism share genus and special traits I fail to make any distinctions between them. Certainly, whatever special epistemology Mr. Germino embraces, he recognizes that many things share genus traits and are not identical; horses are not zebras although both belong to the same genus. Moreover, creatures can even share species traits and yet not be identical. Lapdogs are not Great Danes. For heuristic and classificatory purposes one may dwell on similarities—but for hunting badgers one does well to dwell on the distinction between Dachshunds and Saint Bernards. One must distinguish between cognitive and pragmatic purposes—between classification and evaluation. Where one doesn't one runs the risk of lapsing into silly judgments. Other reviewers have avoided Mr. Germino's mistake. The reviewer for the *Journal of Politics* simply states that I am "sensitive to the differences" but provide a convincing argument for subtending genus and perhaps species similarities.

In some obscure way, I suppose, Mr. Germino knows some of these things. That his knowledge is not evident in his review is, I conjecture, a consequence of his commitment to a new political style (which I have baptized with my own neologism): neo-visceralism.

A. James Gregor
University of California, Berkeley

To the Editor:

The widespread use of statistics in political science (and the other social sciences) undoubtedly is contributing and will continue to contribute to its maturation as a science. This does not mean, however, that their indiscriminate use is wise, for the misapplication of such manipulations can produce results that are highly misleading. The recent article (June 1970) by Brian R. Fry and Richard F. Winters on "The Politics of Redistribution" may be taken as a case in point. Following the earlier work of Richard Dawson and James Robinson, Thomas Dye, Ira Sharkansky, and others, their study tries to answer the question, "Does politics make a difference in the policy formation process?" While it is obvious that in a broad sense politics is very germane to the process, statistical results that confirm the obvious among the American states have not been as readily produced as one might expect. Fry and Winters, using an ingenious measure of "redistribution" (in terms of expenditure as opposed to tax burdens) toward the lower income classes as their "policy" issue, argue that they have proof that "political variables" (including extent of political participation, interparty competition, aspects of elite behavior, etc.) are more important the socio-economic variables (median income, industrialization, urbanization, education).

To prove their argument they test eighteen hypotheses (involving six socio-economic variables and twelve political variables), first by running zero-order correlations between the "redistribution ratio" and each of the eighteen variables, and second by running partials in which they control for the 17 other variables. If one looks just at the zero-order relationships, it is apparent that they have achieved some success (not complete, but some), although some of the correlations are of a low magnitude. While their successes are noted, if only in passing, they pass on to the partial relationships. They state: "Shifting our attention to partial coefficients of correlation, *a measure of independent impact of each independent variable* on redistribution, we find somewhat longer lists of contrary correlations (my italics)." Indeed the contrary correlations include negative relationships between the redistribution ratio and (1) median income (originally .18, when partialled it became −.27), and (2) industrialization (originally .29, it became −.02). To at least some of us this seems most surprising. But if one looks for an explanation of such results, Fry and Winters fail to provide one. A reason for this, it may be suggested, is that such findings are spurious, hence attempting to explain them could lead to strained mental contortions.

The reason that such findings are probably spurious (as well as some of the other findings that are a result of partial relationships, although in a less obvious way; the probability that the "independent" variables are more highly correlated to one another than to the "dependent" variable does not bode well) is that the independent variables in the set of socio-economic variables suffer from multicollinearity (highly correlated independent variables), so that when one "controls" for variation caused by, say, industrialization, urbanization and education to see if "median income" has any "independent" effect on "redistribution ratios," one actually controls the *valid* variation of, in this case, "median income." (Several methodologists have written about this (including Robert A. Gordon, "Issues in Multiple Regression," *American Journal of Sociology*, 73 (1968), 592–616, and Hubert M. Blalock, Jr., "Correlated Independent Variables: The Problem of Multicollinearity," *Social Forces,* 42 (1963), 233–237), Robert Gordon's article being particularly useful.) For example, using Dye's results for the 50 states (Fry and Winter use 48; data are from *Politics, Economics. and The Public,* Chicago: Rand McNally, 1966), there is a zero-order correlation between median income and urbanization of .67, indicating that they are nearly equivalent for the purposes at hand. And, as Gordon notes, "When two variables are equivalent, they will both be equally valid to some degree, and controlling for one of them amounts to controlling for valid covariation. This makes as much sense as controlling for a parallel form of the same instrument." For reasons that cannot be gone into here for lack of space, but which Gordon discusses at length, such partialling can produce weird results. Such, it may be suggested, as those Fry and Winters report.

Probably at the heart of the problem of the use of statistics in such a problem (are political or socio-economic variables more important?) is the belief (see italicized part of quote from Fry and Winters above) that partialling provides "a measure of (the) independent impact of each independent variable. . . ." As Gordon notes, this view is "generally accepted," but incorrect insofar as multicollinearity exists. This letter, then, only attempts to point out for the unwary some of the pitfalls that Gordon tries to indicate and explain.

What implications does this analysis have for the other findings reported by Fry and Winters, including "The most interesting and significant finding in this study (concerning) the relative importance of political and socio-economic variables in determining redistributive fiscal policies in the states," namely that "politics plays a dominant role in the allocation of the burdens and benefits of public policies"? One can probably conclude, it may be suggested, that the other relationships are spurious too. In this section of their paper one may note that Fry and Winters' multiple regression methodology has far outdistanced their theory. They provide, that is, no explanation of, have no theory covering, the overall results of using, first, all eighteen, then "only" ten, variables in a multiple regression analysis, as opposed to their original hypotheses. More controlled investigation may have asked, upon finding a correlation between *a* certain political variable and the redistribution ratio, whether such a relationship was spurious, "reflecting" genuinely the effect of the socio-economic evironment (assuming a correlation between the political variables and the socio-economic environment), and controlled *just* for socio-economic environment to find out if the political variables did have an independent effect. It may be pertinent to note here that their re-analysis using "only" ten variables in a multiple regression equation, where they find also that political variables are much more important than socio-economic variables, could have been expected if the socio-economic variables are more highly intercorrelated, as one suspects is the case, although Fry and Winters do not provide such data. (Gordon's article is very useful in this connection also.)

While Fry and Winters have introduced a useful policy consideration by way of their "redistribution ratio" for future analysts to consider, because of, apparently, multicollinearity among (some of) their independent variables, as well as allowing methodological possibilities to overshoot their theory, their method of analysis and of course their results (past the zero-order correlations) are most questionable. It is perhaps only by making and then becoming aware of such mistakes, however, that understanding increases.

James J. Noell
Washington University, St. Louis

To the Editor:

Mr. Noell's major point is well taken, namely, that multicollinearity is a serious problem in multivariate analysis where high inter-correlations among variables can lead to great difficulties in interpretation of, and misleading inferences can be drawn from, the data. He is also correct in his assertion that the presence of multicollinearity among the independent variables used in our analysis renders the partial correlations difficult to interpret and susceptible to misleading inferences. However, the misleading inferences to which Mr. Noell refers are his, not ours. Briefly, what Noell has done is to use a statistic (the partial correlation) to assess the relative explanatory value of political and socio-economic variables, a purpose for which it is neither particularly well-suited nor intended, while ignoring the statistic (the multiple-partial coefficient of correlation) which we had intended for use in such an assessment. The partial coefficients of correlation were reported simply to indicate the "marginal" relationship between each independent variable and the dependent variable controlling for all other variables in the set with the specific acknowledgement that the complex interrelationships among the individual independent variables would require further investigation (an endeavor in which we are currently engaged). The multiple-partials, on the other hand, *were* meant to be an indicator of the relative explanatory power of the political and socio-economic variables. In employing the multiple-partials for this purpose, we feel that we have substantially reduced, though admittedly have not eliminated, the problem of multicollinearity and consequent misinterpretations of our findings. In sum our purpose was to present a dependent variable of potential theoretical utility in political analysis. It was not our purpose to explore specific causal linkages in pursuit of a general budgetary theory. We feel that the major finding of the paper still stands: measures of economic capability and need were less highly related to differences among the states in budgetary redistribution than were a set of political variables.

Brian R. Fry

Stanford University

Richard Winters

Dartmouth College

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BOOK REVIEWS

Political Change in Britain: Forces Shaping Electoral Choice. By David Butler and Donald Stokes. (New York: St. Martin's press, 1970. Pp. 516.)

Students of voting behaviour, whether or not interested in British politics, will certainly be interested in *Political Change in Britain,* because it is the first book-length analysis of voting behaviour to appear from the Survey Research Center of the University of Michigan since the publication of *The American Voter* in 1960. As a consequence, it is a model of how to analyse voting in a national context, whether in North America, or the Europe of the Six and the Seven. It is also a mine of information about mass political attitudes in Britain.

Unpredictable aspects of elections have given David Butler and Donald Stokes an unusually rich resource of data: nationwide sample surveys of the British electorate at three different points in time, with *two* general elections occurring in the period. Their first wave of interviews was conducted in 1963, when Conservative support was near bottom; the second wave in autumn, 1964, when the two parties were even; and the third after the 1966 general election, when the Labour Party had reached a pinnacle of success. Hence, the authors have a considerable amount of change to analyse by the standards of a relatively stable European electorate. They also have problems of panel mortality, sampling and weighting; these are clearly discussed in an appendix.

The questionnaire employed follows the Survey Research Center's concern with obtaining as much information as possible about individual respondents' social characteristics and political attitudes. Donald Stokes' analytic skills are reflected in the sophistication with which data is analysed, especially where complex problems of cause and effect are concerned, as in the discussion of the relationship of newspaper reading and trade union membership to voting behaviour. David Butler's long involvement in the detailed study of British election campaigns is reflected in the concern shown with the relevance of findings to British electoral politics in the 1960s. A number of key tables are of interest to practitioners of campaigning at Smith Square, London, as well as to an international circle of academic students of voting.

The 20 chapters and 225,000 words of text begin with a straightforward but by no means simple discussion of the social bases of party preference in Britain, with a chapter devoted to religious, local and regional influences, as well as several chapters on class influences and a chapter on socialization and generational differences in party allegiance. The second part is primarily concerned with issues and the extent of ideological awareness. Sections three and four describe influences changing the party preferences of individual voters and the aggregate strength of the parties in the electorate. Here, the availability of data from the same panel for two general elections is particularly valuable.

No review can do justice to all the materials and themes contained within this volume. Moreover, the clarity of organization is such that the busy student of comparative politics can dip into particular chapters and abstract much of use, especially if he is already familiar with the context of British politics. To this reviewer, the most interesting and elegant discussions concerned two topics long emphasized in the Nuffield studies of aggregate electoral data: the evenness of national swing, and the homogeneity of the national electorate.

In a series of seminar papers, Donald Stokes had previously emphasized the paradox of swing: that is, the net percentage change in votes between Conservative and Labour parties in a given constituency is nearly identical in all constituencies in Britain (or, at least, in England). This is a paradox since a swing of 3 per cent of the vote is equivalent to a change of one-tenth of its strength, whereas it reflects the switching of only one-twentieth of strength in a constituency where it polls 60 per cent of the vote. The phenomenon is explained as a consequence of ecological and communication factors that work in opposite directions in a party's safe and hopeless seats, with the net effect homogenizing shifts at the level of the constituency. The discussion blends logic, empirical data and theoretical sophistication.

The decomposition of influences upon voting into their regional components is carried out by using statistical techniques that Donald Stokes has already demonstrated in other contexts. The authors' analysis here shows that 6.4 per cent of the greater strength of the Labour Party in the North of England and of the Conservatives in the South is accounted for by differences of party preference within a given class (i.e., by a 'regional' effect) and only 2.8 per cent is due to the difference in the class structure of the two regions. The interaction effect is a negligible 0.1 per cent.

A pervasive theme of the book is the extent to which actuarial changes in the social structure of the electorate effect the balance of power between the two major parties. This is initially introduced by a careful analysis of generational cohorts, including those undergoing early political socialization when the Liberal Party was a major party, and Labour minor. The analysis extends to consider such matters as the political consequences of differential fertility and mortality rates in the middle and working-class. For example, working-class Conservatives have fewer children than working-class Labour parents. The care and lucidity of the presentation stimulated some British reviewers to hail the book as evidence of an ineluctable trend to Labour. (One satirist even wrote a piece entitled 'The Naked Psephologist'.)

The victory of the Conservatives at the 1970 British general election provides a public rebuke to anyone who ignores the *ceteris paribus* clause in such extrapolations. It also provides the authors with yet another book of major interest, to be based upon the continuation of interviews with a panel of respondents in 1969 and 1970. The desire for replication and intensive analysis of floating voters—where changes are matters of record and not matters of sometimes faulty memory—will presumably lead the authors to devote the bulk of their next study to a further detailed analysis of mass political behaviour and attitudes. It is debatable, however, whether any number of surveys will lead to full comprehension of their titular subject: political change in Britain.

The reasons for this are several and important. The basic question is: What changes when political change occurs? The answer implied by these students of voting behaviour is very different from that presented by Samuel Huntington or Gabriel Almond. The second question is: What time span must be observed to verify that alterations are something more than a transitory quasi-random fluctuation? The answer is likely to be a time span rather longer than the six years (or was it six days?) wonder of Mr. Wilson. The authors' review of the life history of their respondents only underlines the importance of the last big change in British electoral politics: the replacement of the Liberals by Labour. A forthcoming study by Donald Stokes of the relationship of MPs and constituents' attitudes in Britain will provide some insight into the contemporaneous relationship of two significant political strata. But this data cannot analyse possible long-term changes in MP-constituent relationships. Moreover, the peculiarly constrained role of British MPs in the political executive makes it difficult to draw inferences about government from information about MPs.

To say this is not to criticize the book that David Butler and Donald Stokes have written, but to note that the title they chose could equally well have been used for other kinds of political analysis too. In England, this could extend back to times before the introduction of the democratic franchise, to issues and administrative problems remote from the perception of the ordinary voter, and to a universe as confined and elitist as theirs is broad and democratic.

RICHARD ROSE
University of Strathclyde

Obligations: Essays on Disobedience, War, and Citizenship. BY MICHAEL WALZER. (Cambridge: Harvard University Press, 1970, Pp. 244. $7.95.)

It has been the case historically that political theory has flourished during periods of political and social upheaval. The names of such thinkers as Plato, Hobbes, Locke, and Marx, to name only a few, come readily to mind in this connection. In particular, questions dealing with the theory of political obligation tend to assume great importance in such times. There arises a pressing need to explore the conditions under which men are obligated to obey the laws or, conversely, the circumstances under which they *may* disobey them, or—in extreme circumstances—under which they even incur a positive obligation to disobey. Given the present political situation in the United States, it is therefore not surprising that the past several years have witnessed a substantial revival in political theory dealing with these and related topics. Michael Walzer's new book is a distinguished contribution to that revival.

His subjects are of the most immediate importance, including, among others: the nature and demands of modern citizenship; political alienation, conscientious objection and their relation to the system of conscription; civil disobedience and corporate authority; the responsibility of intellectuals; and the obligation to die—or sometimes to live—for the state. The form of the book is that of a collection of essays, but this is not just another typical example of that much abused genre. Over half of the eleven articles have never before appeared in print and the author makes it quite clear that the decision to publish essays rather than a more systematic treatment of the subject was a carefully considered choice related to the considerable number of "kinds and degrees" (p. xvi) of obligation which he is able to distinguish. Thus the papers must be read as tentative explorations and it would be improper to attribute to Walzer the sort of general theory of obligation he so carefully eschews. However, it is fair to say that there are a number of themes which recur with some frequency and to which special importance must be attached.

These papers are applications of a theory of consent derived from such thinkers as Hobbes, Locke, and Rousseau, and even though Walzer is not uncritical of this tradition his work still may be said to represent a revival of social contract theory. In applying this body of ideas there is much emphasis placed upon the need for consent to be given frequently and over an extended period of time, as well as on the real necessity for genuine evidence to be discovered that *meaningful* consent has in fact been granted. Ritualistic formulas are not enough and Walzer displays a healthy skepticism as to the efficacy of many of the mechanisms by which modern democracies attempt to evoke such consent.

A third recurrent theme is the pluralistic basis of political obligation although this pluralism is much more than the "pleasant illusion" (p. 205) so commonly found in contemporary American thought. In Walzer's view, one generally incurs obligations not simply on the basis of the dictates of the individual conscience, not on the Lutheran ground, "Here I stand; I can do no other," but rather through participation in the activities of political or social groups of one kind or another. As Walzer puts it, ". . . commitments to principle are usually also commitments to other men, from whom or with whom the principles have been learned and by whom they are enforced." (p. 5) Moreover, and very importantly, the process by which obligations are assumed is not automatic; instead it stems only from "willful membership" in a group. (p. 7) We have here a restatement of the central point of social contract theory. Under these conditions the pluralist citizen "receives protection and shares in ruling and being ruled, not in spite of his plural memberships, but because of them." (p. 227) Obligations to the state can arise only when the citizen is afforded a choice between the commitments he has made to the groups of which he is a member and the commitments which the state may ask him to make. Only then can citizenship as a genuine moral choice rather than a mere legal status be said to exist.

If there is a serious objection to Walzer's book it would seem to lie in his failure to confront consent theory with other conceivable bases of obligation. For example, in the essay on "Political Alienation and Military Service" he raises a possible objection to his own view which he derives from Blackstone and which holds that "obligation in fact derives from birth and upbringing, and that the voluntary actions or inactions of adults have no significance in the formation or destruction of moral bonds." (p. 115) Walzer admits that this is a position which has consistency and force but he declines to discuss it on the ground that it is "simply a denial of consent theory." (*Ibid.*) This is true, of course, but it will not satisfy advocates of the view that obligations derive from the fact that the state is in some sense a natural institution rather than an artifact of man's will produced by a consensual process. This is the price Walzer pays for choosing to apply an assumed body of ideas to a series of specific problems rather than attempt a more general theory. However, this should by no means be taken to mean that his essays are not by and large very compelling; it is merely to raise a hope that sometime he will turn his attentions to such issues.

Political theorists will learn much from this book and so should many non-theorists as well. To them it offers not only a series of penetrating analyses of currently important problems, but also an outstanding example of that working out of a tradition of thought which Sheldon Wolin has argued is so vital to the vocation of political theory. Anyone operating under the delusion that the tradition is played out should quickly be disabused of that notion by a reading of these stimulating studies.

JAMES P. YOUNG
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The Logic of Comparative Social Inquiry. BY ADAM PRZEWORSKI AND HENRY TEUNE. (New York: John Wiley and Sons, Inc., 1970. Pp. 153. $8.50.)

It is now apparent that the revolution in comparative politics introduced a decade ago by the appearance of *Politics of the Developing Areas* is completed. The functional categories set forth in that volume stimulated a growth of interest in truly comparative research. But ten years of toil have shown the scholar that the important epistemological, theoretical, and methodological problems have yet to be tackled. The work under review here begins that task.

The Logic of Comparative Social Inquiry grows out of the authors' participation in the International Studies of Values in Politics project. It is a short and complex book, often difficult to understand on first reading. Nevertheless, every political scientist who professes to be a comparative analyst should master it; no doctoral candidate can consider himself well trained unless he has perused it thoroughly.

The volume is divided into two main parts. The first, well over half, is concerned with theory building and the second with problems of cross-cultural measurement. There are several interesting assertions about the nature of comparative research. While it is generally recognized that the primary goal of comparative research is the building and testing of theories, the authors postulate that generality and parsimony of theories is more important than accuracy. Even more significant is the rather special meaning given to the term "comparative" which is reserved exclusively to denote studies "in which the influence of larger systems upon the characteristics of units within them is examined. . . . In this sense not all the studies conducted across systems or nations are comparative. . . . If the analysis is conducted exclusively at the level of nations, then . . . it is not comparative." (p. 74)

This is an important departure from the normal usage of the term "comparison" in the social sciences ("comparative psychology" excepted). In the tradition of Bacon, Mill, Weber, Durkheim, Nadel and many others the comparative method was another form of the experimental method. Instead of being able to control and manipulate variables in a laboratory, the social scientist had to seek out natural social settings. He would test or generate hypotheses by comparison across cultures in which his theoretical variables occurred in the most useful combinations for causal inference. Thus Weber, through an "imaginary experiment," compared Western and Indian cultures to derive his famous hypotheses on the relationship between religion and capitalism. And, Nadel compared similar societies in Africa to generate propositions on witchcraft.

Przeworski and Teune point out that the old comparative method, in addition to limiting the researcher to natural settings and introducing problems of diffusion, was based on the unrealistic assumption that characteristics of cultures could be manipulated in quasi-experimental fashion. It was also fairly rigid—the a priori assumption about the importance of certain factors had to be followed through to the end of the study.

In place of this they recommend a "most different systems design" in which the question of explaining variation remains open throughout the process of inquiry. Thus it is possible to move from the level of individuals, to groups, to communities, to regions and on to total countries. Whatever level produces the greatest reduction in variance and thus provides better prediction is the one used in the final analysis. For them "Comparative research is inquiry in which more than one level of analysis is possible. . . . a study of local leaders sampled from local communities in a single country is comparative since research can proceed at both the individual and community levels." (p. 37)

Part I is a discussion of the various facets of this research design. It is replete with illuminating examples from comparative research. Throughout the work the unit of analysis most frequently referred to is the "system." The goal is to replace the proper names of systems (e.g. India) with the names of theoretical variables. In theory formulation proper names become the final residual concepts explaining the remaining variance in the dependent phenomenon.

One difficulty is that the reader is never told explicitly how to identify the various levels and units discussed. What is an "individual," a group, a community, or a country? What is the epistemological status of these concepts? How are they related to each other? Although a formal definition of "system" is provided, what are its operational indicators? How is one to know that a certain level in one system is comparable to a level in another? The entire discussion of changing levels of analysis was difficult to understand for this reviewer and yet it is a key element in the argument.

Part II consists of a good survey of the problems of measurement in cross-cultural research, particularly as applicable to the design set out previously. The problem is presented in the form of developing a meta-language which will provide equivalence of measurement across systems. There are some surprising assertions here too—such as that stimulus equivalence is not as important a problem in designing a questionnaire as might be supposed.

The two main parts of the book are not as well integrated as they could be. Nevertheless, the volume is an important addition to the recent literature on comparative political analysis. The problems it raises are not always settled, but it is a good beginning.

W. G. Fleming

New York University

Protest in City Politics: Rent Strikes, Housing, and the Power of the Poor. By Michael Lipsky. (Chicago: Rand McNally, 1970. Pp. 214. $5.95.)

Some of the central themes of Michael Lipsky's *Protest and City Politics* will be familiar to readers of this *Review* (December 1968) but even those who have seen the preview will find the book provocative and worthy of examination. Professor Lipsky seeks to increase our understanding of both low cost housing problems and the dynamics of protest but the latter is clearly his prime concern. As Lipsky acknowledges, he is concerned with only a segment of that which is often described as protest: i.e., nonviolent political action involving showmanship or public display by groups he (rather vaguely) defines as "relatively powerless" that is aimed at obtaining changes by and through the established economic and political systems. One may, of course, wish that he had defined his interests more broadly but the territory he has staked out is clearly in need of exploration.

Professor Lipsky argues that there are limits to the effectiveness of protest activity, especially recurrent protest by a particular interest, and that these limits may best be understood by focusing on the problems faced by protest leaders in managing the various constituencies from which support must be secured. Since the needs of the target agency, or group, third parties, the mass media, or members of the protest group itself are often in conflict, the position of the protest leader and the stability of the movement are inherently tenuous.

As Professor Lipsky sees it, a necessary if not sufficient condition for successful protest is the mobilization of third parties to which the target group refers (or defers) and the mass media is seen as essential in such activation. Certainly, the support of third parties and sympathetic treatment by the mass media can increase the probabilities that protest will succeed. On the other hand, protest by "relatively powerless groups" can be direct and neglect by the media need not be fatal. For example, protests led by James Forman and the Black Economic Development Conference appear to have secured substantial funds from a number of churches without the involvement of third parties. And, Forman's "reparation" demands did not generally receive friendly response from the media. Moreover, if the referent groups of protest targets (i.e., third parties) are elites, as they most often are, it seems likely that direct contact by protest groups will suffice in many cases to explain protest demands. It may be that (1) as those who would utilize protest as a political resource have come to recognize Professor Lipsky's point that successful appeals to third parties and to the media are likely to complicate the task of maintaining the cohesion of the parties to the protest, and (2) as the nature of demands by racial minorities and the poor have changed, the role earlier played by third parties and the media in successful protest has diminished.

One of the difficulties with Professor Lipsky's approach is that groups utilizing protest may also make use of strategies such as legal action, direct lobbying and other conventional forms of interest group activity. No effort is made to sort out the contributions the various strategies make to whatever success the protest group experiences nor does he warn us of the problems involved in doing this. It seems possible, for example, that protest groups can be effective despite, rather than because of, their protest activity.

Lipsky deals with the difficult problem of assessing protest effectiveness with sophistication and admirable thoroughness. While he does not show what factors actually brought about or discouraged response to protest demands, he makes an important distinction between symbolic responses and policy changes on the one hand, and direct benefits on ths other, and rather convincingly demonstrates that rent strikes secured very little of the latter. Among the explanations suggested for why protest groups are more successful in achieving policy innovation than in altering administrative procedures or securing direct personal remedies to grievances are: 1) policy changes are less costly and more interesting to city officials; 2) third parties are more likely to seek seemingly dramatic policy changes than more short-run responses to protestors' grievances; and 3) protest groups are inherently more effective in raising issues and establishing their importance than in participating in the formulation or adoption of solutions to the problems they dramatize. Perhaps identification of the barriers to effective protest also might be facilitated by looking as much at what protest targets are capable of doing as at what they actually do.

While few will agree with every aspect of Professor Lipsky's formulation of the process of protest, most will find food for thought in the numerous generalizations he advances. The value of his book, however, does not rest solely on its more abstract formulations. The reader will find in the descriptive segments of the book numerous insights many of which are deserving in themselves of further analysis. His commentary is quite strong with respect to the response of formal institutions to demands for housing reform. The discussion of the diversionary tactics utilized by city bureaucrats is especially penetrating (though one wishes more could be said about the incentives bureaucrats have for such obfuscation). Lipsky's examination of how the demands of the protestors were formulated or how Jesse Grey (the rent strike leader) came to dominate the protest and how he handled the problems he is said to have faced in dealing with his indigenous constituents seems less adequate.

Professor Lipsky views his analysis as a critique, or at least a modification, of those theories which seem to stress the widespread opportunities for participation in decision-making available in our political system. It seems doubtful that many among us, including the most sanguine pluralist, will disagree with his conclusion that the poor face unusual difficulties in mobilizing effective political action. However, Professor Lipsky may be mistaken in explaining so many of the difficulties in terms of the fact that the poor lack organizational skills and the "status and refinements which accompany income, education, and social position." Perhaps it is not only, or even most importantly, that the poor are less well organized and less politically sophisticated than the middle and upper class but that they demand more basic change. No matter how articulate and well organized they may be, the advocates of fundamental change will invariably face more substantial constraints than groups that place less substantial demands on the prevailing allocation of resources and privilege.

Finally, one of the refreshing things about Professor Lipsky's book is that it manifests a clear and uncommon sense of what politics is about. Whatever its methodological weaknesses (the limits of the case study are apparent here), whatever modification to its central conceptualizations may be warranted, the book conveys the complexities and ambiguities of political action and this seems reason enough for including it on one's list of things to read.

Willis D. Hawley

Yale University

The End of Liberalism: Ideology, Policy, and the Crisis of Public Authority. By Theodore J. Lowi. (New York: W. W. Norton, 1969. Pp. 322. $6.95.)

Professor Lowi has an argument with America and with American political science. The American liberal state, he argues, is not working. Its ideology is outmoded, its institutions are incompetent, and its policies unjust. Pluralism and interest-group liberalism have failed, and their failure has produced a crisis of authority which now threatens the foundations of order. At the same time, the political science which claims to study that state has become its ideological handmaiden. The crisis is academic as well as practical.

Lowi's earlier critiques of pluralism and interest-group liberalism are by now familiar to students of American politics. The present book represents an empirical broadening of that earlier work, and a polemical sharpening of it, but the intellectual and political framework remains the same. The work stands squarely within the neo-liberal tradition even while criticizing it. *The End* is both a major expression of American liberalism—the most important since Robert Dahl's *Preface to Democratic Theory*—and a vigorous critique of it.

Lowi's primary thesis is that "Interest-group liberalism seeks to justify power and to end the crisis of public authority by avoiding law and by parceling out to private parties the power to make public policy. A most maladaptive political formula, it will inevitably exacerbate rather than end the crisis. . . ." (p. 58). In developing this thesis, Lowi builds a formidable bill of particulars against the regime of interest-group liberalism. Interest-group liberalism corrupts democracy, renders government impotent, and cannot achieve justice. The liberal state degrades general law into universal privilege. Forms and formalisms disappear, replaced by temporary bargains arranged by the interested parties themselves, and then imposed on the rest of us for our distrustful submission. Awash in relativism, buffeted by a thousand groups and interests, liberal states cannot plan for the future or steer a safe course through the hazards of our time. The pluralist model of the self-correcting system is a sham. It was based on impossible assumptions about free group competition and consensus on the rules of the game, and it excluded large groups from most of the benefits of society. It concealed the real nature of interest groupings under a cloud of sentimental rhetoric, so that "pluralist theory became the complete handmaiden of interest-group liberalism . . . much more than laissez-faire economics was ever a handmaiden to big capitalism." (p. 296).

While none of the specific charges in this case is original, the whole is impressive for its scope and coherence. If Hegel was right about the time of day when Minerva's owl takes flight, Lowi's book suggests that liberalism has reached its late afternoon.

Lowi exhibits and supports these assertions in a series of studies on various institutions and policy areas. This, the substantive, empirical part of the book, rests on solid, forceful, and discriminating scholarship. The contrast between "old" and "new" welfare is shown to be a contrast between a program which knew what problems it was coping with and one which conceals confusion under the slogans of experimentalism and participation. Urban renewal and federal housing, under the liberal dispensation, have become obscene caricatures of social justice, fattening the real estate interests, subsidizing the flight of the whites to the suburbs, and institutionalizing a form of apartheid. Blocs of farmers set agricultural policy. The Departments of Commerce and Labor are thoroughly in the grip of their clientele groups.

In all these cases the same process repeats itself. Under the ideology of pluralism, with its faith that the public good emerges from the clash of private interests, public power is parceled out to special interests. Democratic responsibility, general standards of policy, and uniform procedures of administration decline. In their place appears something fearfully similar to the corporate state, where organized and irresponsible interests capture the power to govern and set both the direction and the details of public policy. As the gap between liberal ideals of justice and the realities of organized selfishness widens, more and more people become alientated. The crisis of authority grows, and it will not be stopped by attempts to buy consensus.

After the diagnosis, Lowi bravely offers a cure. What the people want is not bribes and special privileges, but standards of conduct they can live up to established by an "independent and legitimate government" (p. 297) they can respect. Toward this end, he proposes a "radical platform" of "fundamental institutional change." The main planks are (pp. 297–310): stop large and vague delegations of power by restoring the *Schechter* rule; centralize administration and require early and frequent administrative rule-making; foster "a truly independent and integrated administrative class—a Senior Civil Service;" abolish city corporate autonomy; strengthen the federal battery of fiscal regulatory powers and techniques; set a "limit of from five to ten years on the life of every organic act." By such "radical" means would Lowi establish a new political formula, called "Juridicial Democracy."

Herbert Croly wrote *The Promise of American Life* sixty years before Lowi wrote *The End of Liberalism,* but *The End* surely fulfills the spirit of *The Promise.* All of Croly's dominant themes are here—from the attack against the special interests, through the assault on the shibboleths of democratic participation, and on to the passion for centralism, efficiency, and the administrative elite: powerful officials directing a powerful government toward the public interest. Both are convinced that reliance on "the people" really means victimization by organized and aggressively selfish elites. And if the people are not at the foundation of political life, then an elite must be at the apex. All that is missing is Croly's nationalist rhetoric.

In sum, while Lowi offers a useful analysis of some of the flaws in the liberal-corporatist regime, his analysis stays well within the assumptions of such a regime, while his prescriptions intensify its most dangerous tendencies. Let me illustrate these contentions.

The crisis of authority is deeper than the New Deal consolidation of 1936–39. It has its roots in the secularism, relativism, and materialism of our time, and it is nourished by the very logics of efficiency, rationality, and centralism which Lowi applauds. The crisis has long been manifest in the family, religion, educational institutions, and work. Restoration of the *Schechter* rule will be of little help, for the terrors and predicaments of our time are deeper than Lowi's orientation can acknowledge. The United States is already well into a period of conflict over fundamental questions—a period of great politics, in Tocqueville's expression. Lowi's book, by its silences as well as its assertions, still sees our condition as one of little politics, in which interests are the substance of political life and efficient procedures the concern of political thought.

Lowi's prescriptions—basically the rule of law plus great power in the hands of an expert national elite—are as dangerous as the disease. At one point (pp. 158–160), he argues that the American response to crisis situations in foreign affairs is good because "crisis decisions in foreign policy are made by an elite of formal officeholders" who can "act rationally" because they are insulated against political pressures. As examples of "exemplary behavior in crisis" he offers the Truman Doctrine, the Berlin Airlift, response to the Korean invasion, the 1956 Arab-Israeli intervention, the Cuban missile crisis, and our response to Dienbienphu in 1954. He might have added Hiroshima and Nagasaki, the Bay of Pigs, the Dominican Republic, Vietnam-Cambodia-Laos. One who can keep his faith in "an elite of formal officeholders" after such decisions is in no danger of ever losing it.

This book, I think, shares the liberal fear of politics and the inability to see that the politics of a free people both depends upon and promises more than a machinery of offices, procedures, statutes, and programs. The centralism, formalism, and elitism recommended throughout the book will not save us, though they may give our fears a restless peace. Nor will the Supreme Court prayed to in the book's last paragraph, though it may give our hopes a mortgaged home.

Despite my uneasinesses with *The End of Liberalism,* the reading of it was an intellectual pleasure. Professor Lowi has written a serious work on an important subject. It deserves to be widely read and discussed. It should open a vigorous conversation among students of American politics.

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Conflict of Interest: A Theory of Divergent Goals with Applications to Politics. BY ROBERT AXELROD. (Chicago: Markham Publishing Company, 1970. Pp. 216. $8.95.)

Conflict—its bases, manifestations, and resolution—is the stuff of politics. Whether as interpersonal persuasion, competition, or coercion, conflicts over the authoritative distribution of resources and its underlying values define the essence of politics. Professor Robert Axelrod contributes to the formal analysis of this "stuff" in his recent book *Conflict of Interest*. Though his examination is elementary, his treatment of conflict of interest is careful, considered, and eminently readable. More importantly, it is a fine example of the use of careful, formal reasoning in the treatment of an especially relevant topic.

Professor Axelrod's study is useful for a number of reasons. The most glaringly apparent of these is the obvious void it fills. Political scientists are neither ignorant of the importance of conflict in their inquires nor hesitant to venture into the field to study its various manifestations. However, with the noticeable exception of those interested in game theory and collective decision-making, it does seem that political scientists have devoted a minimal amount of intellectual energy to definitional problems (the magic word, I think, is "conceptualization"). Axelrod begins this task.

In the first part of the book, Axelrod attends to theoretical questions. A simple theme dominates the discussion, namely that ". . . the amount of conflict of interest in a situation of strategic interaction can be used to predict certain aspects of the behavior of the actors in that situation" (p. 5). In particular, Axelrod argues that conflictual behavior is related to the amount of conflict of interest in a contingency, and that the former may be explained by the latter. Taking conflict of interest to mean incompatibility of the actors' preferences, he suggests a measure of the concept which is independent of measures of the behavior to be explained.

His initial political paradigm is the two-person bargaining game. For this class of games he proposes five properties (axioms) that a measure of conflict of interest should satisfy. Axelrod then proves that one and only one technical definition is consistent with the five properties. The axioms, similar to those found in game theory and information theory, are:

1. Symmetry with respect to the labeling of players
2. Invariance under utility transformations
3 Continuity with respect to the strategic environment
4. Boundedness
5. Additivity

At this point it is useful to ask how a measure such as Axelrod's (and, in effect, the axioms or properties which imply it) might be evaluated. Axioms are funny things and social scientists often misunderstand their purpose. Axioms are not statements about reality. They are analytical constructs and their empirical content may be irrelevant. The criteria for judging the efficacy of axioms are the degree to which they account for empirical regularities and their fruitfulness in other theoretical enterprises, not their "realism." As to the fruitfulness of Axelrod's axioms in other enterprises, the first four are relatively uncontroversial, although Schelling has occasionally been critical of the first and third. The fifth axiom, additivity, is somewhat more controversial. In essence it asserts that the conflict of interest in a game is the sum of the conflicts of interest in the subgames composing it. The controversial feature of this axiom involves the elimination of strategic interaction among subgames. Although Axelroad acknowledges the restrictiveness of this axiom and seems to be bothered by it, I find it difficult to make *a priori* judgments. The utility of axioms is best determined by the "quality" of their implications. The applications of the conflict of interest measure provide an opportunity to make this evaluation.

The second part of the book, devoted to applications, illustrates the strengths and the weaknesses of the analysis. The applications presented here involve conflict situations in the congressional conference committee, multilevel bureaucracies, electoral competition, and legislative coalition-building. The models he considers are simple, yet they demonstrate the versatility of his index. They also shed light on the suitability of the axioms as defining characteristics of conflict of interest. This is perhaps best illustrated by his treatment of electoral competition in pluralistic democracies.

Professor Axelrod employs the Hotelling-Downs spatial model of electoral competition in order to generalize his conflict-of-interest concept. In the unidimensional version of this model, voters have single-peaked preference functions defined on a policy dimension. Axelrod assumes, unnecessarily I think, that utility loss for a voter is linear with distance from his most-preferred point. Given this arrangement he shows that the conflict of interest between any two voters is proportional to the square of the distance between their most-preferred points. He then defines "conflict of interest in society on a given policy dimension" as "the average conflict of interest between a pair of people, as each one of the pair takes on all the positions in the policy dimension in proportion to the position's frequency in the society" (p. 150). That is, social conflict is defined as a weighted average of dyadic conflicts. With this definition and the earlier result, he proves that "conflict of interest in society measures exactly the same thing as does the variance of the distribution [of voter most-preferred points]. Hence the concepts may be used interchangeably" (p. 151).

At this point some peculiarities arise. Consider the case of two polarized electorates possessing identical voter distributions. In one electorate each voter's utility function slopes gently in either direction from his most-preferred point. In the second electorate, on the other hand, each voter possesses a very "spiked" utility function. That is, in the former electorate preferences are much less intensely held than in the latter. Intuitively, it would appear that the conflict of interest (and hence conflictual behavior) in the electorate with intense preferences is higher. Yet Axelrod's index assigns equal conflict of interest to each. In my judgment, then, Axelrod's axioms are not complete for they do not discriminate among contingencies with varying intensity.

A second weakness in the index follows as a result of the second axiom. This axiom requires the index to be invariant under utility transformations so that normalized versions of conflict situations may be considered. However, this restriction, desirable for other reasons, does not enable the researcher to discriminate varying saliency. With Axelrod's index conflict of interest may be high in certain issue areas, but conflictual behavior absent because the issues are trivial. On the other hand, an issue area, e.g. gun control legislation, may generate low conflict of interest owing to a large majority on one side of the issue, yet encourage conflictual behavior because of the saliency and intensity of preference of a small minority, e.g. National Rifle Association.

These omissions point to the need for a more complex analysis of conflict. Saliency and intensity are concepts deserving of elaboration. Nonetheless Axelrod's treatment is commendable for steering us back to fundamentals. Conflict, he reminds us, is a function of *expectations about outcomes*. This point is frequently overlooked in the social scientist's concern with process.

Conflict of Interest, as well as other studies of the same genre, convincingly demonstrate the power of formal analysis. The insights (as well as the omissions) should generate a productive dialogue.

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Regionalism in American Politics. IRA SHARKANSKY. (New York: Bobbs-Merrill Company, 1970. Pp. 194. $9.50.)

In what will unquestionably become a reference point in the literature on regionalism, state politics and policy analysis, Professor Sharkansky employs an impressive assortment of statistical tools to analyze the economic and regional correlates of no less than 61 political and policy variables. Following the contiguity principle, he uses four demarcations to divide the 48 states (Alaska and Hawaii are excluded) into 17 regions. Later in the book, an eighteenth region is added. Three indicators of state economic characteristics are used—per capita personal income, urbanization and total personal income. Professor Sharkansky exhibits an extraordinary amount of courage in embarking on such an unwieldy task in an effort to examine such an elusive concept.

The purpose of the book is to provide answers to four basic questions. The first two questions are closely related. They are:

1) How do state politics and public policies of each region differ?
2) How consistent are the patterns within each region—that is, what diversities in politics and public policies exist within each region?

The answers provided for these questions, as the author warns, are indeed confusing. In part, the ambiguities in these chapters can be attributed to the complexity of the data. There are, in addition, some interpretive problems.

In Table 3–2 (p. 67), the author compares the intra-regional uniformity of each of the seventeen regions. Coefficients of variability (which measure the magnitude of variation around each regional mean; are used as the measure of uniformity on each of the 61 dependent variables. In this table he lists the region and the number of coefficients of variability in each which were less than 0.1. The author concludes that

> . . . regions tend to be more uniform in their political traits than is the nation as a whole; in the case of every dependent variable, most of the regions show smaller coefficients of variability than does the aggregate of 48 states.

He adds that

> for only ten of the 61 variables do more than one-third of the regions show greater diffusion (relative to their means) than the nation as a whole.

These conclusions appear to be very tenuous because the analysis of regional uniformity suggests that there is only a relatively small proportion of the 61 policy variables which can be considered uniform in each of the regions. The most homogeneous region, the Upper Middle West, provides only 15 out of a possible 61 coefficients of variability of less than 0.1. This means that on the remaining 46 variables there was no uniformity within the region. This number drops to 14 for the next most homogeneous region, the Great Lakes, and then falls off to 11 for the third—the Mountains. The modal number of homogeneous variables for the regions was only 7. Two regions—the South and the Transmississippi—had only 3 variables which met the requirements of uniformity.

These findings would appear to raise questions about the definitions of uniformity and region. Can clusters of states which are uniform on only 3 of 61 variables be considered regions? If so, what does the concept region mean in a political sense? If not, at what point does a geographic cluster of states become a political region? Moreover, this apparent lack of intra-regional uniformity leads one to wonder whether uniformities at least as convincing could be found in random clusters of non-contiguous states. Even more basic is a question whether a priori definitions of region are appropriate.

There are further ambiguities in the research design. The 17 regional groupings are not mutually exclusive, i.e., the same states are included in different groups. For example, the two most homogeneous regions—the Upper Middle West and the Great Lakes—both include the states of Michigan and Wisconsin. The author notes the similarities between these regions. For example, both regions exhibit high voter turnout, intense party competition, relatively high state and local expenditures and a strong reliance on state and local revenues. Question: Is the uniformity in both regions explained by the inclusion of Michigan and Wisconsin in each?

The two regions are then compared and the author concludes that although the regions are very similar, "the Great Lakes states show somewhat less voter turnout and party competition, a lower level of government spending, less reliance on federal aids and better . . . secondary roads" (p. 68). It would appear that such minor differences cannot be explained as regional peculiarities. Clearly, any differences between these two regions can be attributed to only two of the four states in the Upper Middle West and three of the five states which comprise the Great Lakes region. The lack of regional exclusivity raises questions about many of the inter-regional comparisons made throughout the book.

The third question addressed is:

> 3) To what extent do regional traits in politics and policies merely reflect the regional distribution of economic resources among the states?

Professor Sharkansky employs simple and partial correlation analyses, coefficients of multiple determination and three separate analyses of covariance to answer the question. The results of these analyses are reported in two truly formidable tables in chapter 5. In Table 5–2 the coefficients of multiple determination (R^2) indicate that economic measures account for at least half of the *interstate* variation in only four of the 61 dependent variables. Thus he concludes that "large proportions of interstate variation in political traits are not subject to an economic explanation."

In Table 5–3, the analysis of covariance leads to the conclusion that

> . . . non-economic features are significantly related to fifty measures of state politics and public policy while controlling for the influence of economic conditions; economic characteristics show significant relationships with 57 measures while controlling for other regional attributes (p. 112).

The use of tests of significance to determine the strength of independent variables creates some ambiguities in the interpretation of this table despite a lengthy footnote warning, in part, that "It is not feasible to make direct comparisons of the correlation coefficients from the right to the left side of [the table]." For example, one questions the value of tests of significance as an interpretive device when correlation coefficients as low as .10 are significant at the .05 level.

The final question addressed is:

> 4) For those regional differences in politics and policies that cannot be traced to current economic characteristics, what other experiences shared by neighboring states provide an explanation for regional peculiarities?

The author uses regression analysis to arrive at estimated regional scores on each dependent variable, given the average economic characteristics of states within each region. When these estimates depart from *actual* values by at least 15 percent on a dependent variable, the author speculates about the non-economic factors within each region which may account for this deviation. As the author suggests, a more thorough investigation of the myriad of non-economic factors which may account for the unexplained variance in regional politics and policy is beyond the scope of the book. This chapter does provide, however, a number of hypotheses worthy of future research.

In a tangential chapter (to the four basic questions raised), the author examines regional change during the twentieth century and concludes that "there appears to be a development toward both greater regionalism and greater nationalization . . ." This chapter, however, does not compare well with the author's interesting subsequent research on "incrementalism" as a factor in policy change.

In part, the book is a bit disappointing because it does not meet the high standards of

Professor Sharkansky's other work. The difficulties in the book which have been suggested in this review should not detract from its overall value as an interesting and innovative scholarly effort. Such difficulties are to be expected as new advances are made in any area of scientific inquiry.

JAMES W. CLARKE
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The Political Dilemma of Popular Education: An African Case. BY DAVID B. ABERNETHY. (Stanford: *Stanford University Press*, 1969. Pp. 357. $10.00.)

In newly developing societies, popular education is highly valued both as an end and a means. As an end-in-itself, education realizes the dormant spirit of a people. As a means, it is their main road to collective development and active participation in the modern world culture.

As Professor Abernethy demonstrates in this impeccably researched study, conducted in southern Nigeria, few peoples surpass the southern Nigerians in their zeal for education as both a measure of cultural pride and an investment in social development. With great resolve and firm faith, the two regional governments of southern Nigeria, at the time of national independence for the Nigerian federation as a whole, were devoting more than 40% of their annual recurrent expenditures to education. The lion's share of this outlay was spent on schemes for universal primary education, resulting in the rapid rise of enrollment from 30% of the school-age population to 75% in less than a decade. Should that extraordinary achievement be criticized? Abernethy's critique should be read because it is as sympathetic to the goals of the Nigerian educational planners as it is realistic in its assessment of their programs.

Briefly, Abernethy contends that southern Nigeria spends too much money on educational objectives of questionable relevance to the needs of that society. Specifically, he shows that universal primary education has been pursued at the expense of more directly productive investments in agriculture, industry, and transportation. This might be justified if the system of education were rationally designed to complement the economy and promote its growth. The case, however, is to the contrary. Nigerian popular education, with its primary and literary emphasis, is more of a consumption item or welfare benefit than a social investment. It fails to produce either an adequate supply of the skills required for rapid economic development or the capacity of citizens to lead satisfying agrarian lives. For countries at the Nigerian level of development, an excessive expenditure of funds on education may be unwise from the strictly economic point of view. Abernethy reveals a more serious misallocation of funds within the educational systems of southern Nigeria. That misallocation has helped to generate urban unemployment, widespread personal frustration, and political discontent.

It might be argued that the misallocation of resources in this field was a virtually inevitable consequence of the historic relationship between education and religion. Southern Nigeria's school system was conceived and carried on primarily as a mission enterprise. Over the years, these schools have been financed and supported with intense devotion by parishoners. Today, 80% of all primary school students in this area attend church-affiliated schools.

Abernethy's account of educational expansion, involving an analysis of relations between rival mission movements, is masterful. His evidence substantiates Coleman's thesis to the effect that Nigerian nationalism was largely nurtured by the mission school movement. Naturally, nationalism matured with a sturdy belief in the efficacy of familiar forms of education. To a point, the most familiar form—rote literary learning—was fairly consistent with the needs of a commercial colony. Initially, there was an obvious link between primary education and desirable employment. That link was not obviously broken until after the introduction of universal primary education.

When, in the early 1950's, nationalist politicians took control of the school system, their watchword was expansion of opportunity rather than revision of content. In other words, they were pledged to perfect and enlarge an existing system. Furthermore, the cause of popular education suited their claim to govern in the name of democracy. To suggest that these leaders were destined to misallocate their resources because of circumstances that they could not be expected to see objectively, even less alter purposefully, would not go beyond the evidence in this book. Even the movement for secular education served to strengthen the parochial pillars of the inherited system, since the primary goal of expansion put a premium on the expertise and financial solvency of the religious schools.

It should be noted, that the greatest of African educators, in the tradition of Edward W. Blyden, have always been alert to the shortcomings of colonial-style education. In Africa, as elsewhere, the barrier to progressive change is not a lack of social imagination. Abernethy documents the disposition to change, including a widespread desire for technical education for skilled manual work. The chief obstacle to change would appear to be a pattern of interests that is broadly social in nature, rather than educational or intellectual.

For example, the school system itself is shown to foster the growth of a privileged ruling class. This finding is somewhat paradoxical. It seems that the democratization of primary education has been accompanied by an increasing inequality of opportunity at the point of entry to secondary schools. Government grants-in-aid to secondary schools have actually subsidized the education of those who can afford to pay the relatively high remaining fees required to defray the cost of a good secondary education under existing conditions. As a rule, admission to a secondary grammar school is the key to affluence, newly acquired or enhanced. This matter is discussed cogently in terms of class formation. But the issue of equality is the only one analyzed from that theoretical perspective.

In order to link the educational dilemma with Nigeria's great national crisis, Abernethy resorts to the familiar notion of "political decay," holding, roughly, that runaway mass expectations have lowered resource levels, sapping the capacity of the political system to realize its goals. His overall approach is a composite of functionalism and the idea of "decay," leading to this pithy statement of the dilemma of popular education: "It is both a necessary condition for political development and quite possibly a sufficient condition for political decay." A more venerable language, emphasizing the forces of social change and their fetters, might be preferred and would be consistent with what is said in conclusion about "the creative potential of crisis." Yet this skillful application of various approaches is in itself a noteworthy contribution to the literature of development politics. Indeed, Abernethy has written a basic book, both for students of the relationship between education and development and for students of Nigerian social history. It auspiciously inaugurates a new series of books—the Stanford Studies in Comparative Politics.

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Authoritarian Politics in Modern Society: The Dynamics of Established One-Party Systems. Edited by Samuel P. Huntington and Clement H. Moore. (New York: Basic Books, Inc., 1970. Pp. 533. $12.50.)

The opening thrust of this significant book is a challenge to what Professor Huntington perceives to be the misguided conventional wisdom of the 1960's, the belief that authoritarian governments are about to pass from this earth. Huntington identifies the state of pessimism for the future of authoritarianism and optimism for the inevitability of democracy with a propensity for glib generalizing about the pluralistic requirements of modernization and the leveling and rationalizing effects of industrialization. These views in turn have led to theories and hopeful expectations about the liberalization of Communism, the "convergence" of the superpowers, and the rapid "development" of all the newly emergent states, all of which when taken together would bring a standardized model of democracy to all societies

The issue is overdrawn, particularly given the latitude Huntington takes in his definition of authoritarian governments. "To keep things simple, an authoritarian government is defined as a non-democratic one. A democratic government is one whose principal leaders are chosen at regular intervals through competitive elections in which the bulk of the adult population has the opportunity to participate." With such a definition authoritarianism might have little to do with repressive government and could include everything from bungling, incompetent and ineffectual regimes, to the rule of benign technocrats dedicated to populist values.

In actual fact, however, neither the issue of historical trends nor of the proper definition of authoritarian government are of great importance because the focus of Huntington's introduction and the book as a whole is less on the concept of authoritarianism and more on the character of established one-party systems; that is, the book deals more with its subtitle than its main title.

Personally I found Huntington's initial essay a brilliant and at times dazzling exercise in classifying and distinguishing all conceivable types of one-party systems. Although tightly reasoned and rigorously disciplined, his typologies are not merely the products of semantics and logic but rest instead upon very provocative and penetrating empirical propositions about the relationship between social and class divisions and types of one-party systems. By making the important distinction between "revolutionary" and "exclusionary" one-party systems he demonstrates that internal transformations can come to revolutionary systems without necessarily bringing an end to one-party rule. Huntington outlines the sequential process of "transformation," "consolidation," and "adaptation" that produces the "established one-party system."

Although Huntington is careful not to overstate the advantages of "established one-party" systems for accelerating modernization and development, he recognizes the utility of what he considers to be the key characteristics of such systems for effective development. His list of characteristics again raises the question of the appropriateness of his label of authoritarian government. In his view such regimes stress pragmatism rather than ideology, have oligarchical and bureaucratic rather than charismatic and autocratic leadership, are prone to give initiative for change to a relatively dispersed technocratic and managerial elite, accept the role of interest groups and of intellectuals who may criticize governmental performance. Given these characteristics as the substance of Huntington's expectations of what one-party rule is likely to end up as, it is apparent that he is considerably more optimistic about benign trends than he wants to admit. But again let us repeat that it is important to ignore semantic issues and recognize Huntington's great analytical powers.

Professor Moore's introductory chapter is also an impressive essay that focuses on the problem of legitimacy and ideology in one-party systems. Nearly every sentence reflects detailed familiarity with a wide variety of times and places, and comparison reaches the point at which the reader must mentally jump in the same sentence from Liberia to Virginia, from Turkey to Mexico, from Spain and Portugal to the Soviet Union and Nazi Germany.

Moore seeks to bring order to the variety of single-party ideologies by using a four-fold typology which distinguishes *functions* of ideologies as being either instrumental or expressive and *goals* as being either the total transformation or a partial transformation of the society. Personally I find it extremely difficult in practice to judge whether an ideology is more instrumental or expressive. Can one, for example, really say that the Thoughts of Mao Tse-tung provide instrumental guidance for policy, or that even the earlier Chinese Communist doctrines contained the qualities of a blue-print for action? Similarly I am not sure that we are on very solid ground by saying that ideologically Fascism was more expressive and Naziism more instrumental.

I find more useful Moore's argument that the legitimizing force of one-party ideologies stems from different mixes of three basic dimensions contained in all ideologies: the chiliastic or shared myths, the tutelary or agreed goals, and the administrative or procedural traditions.

As a reviewer I am distressed with the difficulty of doing justice to a symposium volume in which the editors present the general introduction and conclusions and a group of very distinguished scholars provide rich and highly competent country case studies as the bulk of the book. Limitation of space means that it is hardly possible to identify, to say nothing of evaluating, the separate chapters, but I do feel that the minimum requirements of justice demand the recognition that all the authors have made valuable theoretical, and not just descriptive, contributions.

Hugh Douglas Price has done the near impossible by brilliantly compressing existing knowledge about one-party state systems in the United States into a few pages while delineating historical trends. Henry Bienen effectively criticizes early theories about one-party systems in Africa, and arrives at the conclusion that they should be largely understood as analogous to "machine politics" in urban America. Out of his deep knowledge of the workings of Franco's Spain, derived from both personal experience and scholarly study, Juan Linz has produced a rare document on the social dynamics and the intellectual arguments that produced the shift from Falange to Movimiento-Organizacion in Franco's Spain.

Carl J. Fredrich's data-filled summary of the characteristics of Hitler's Germany, and Andrew C. Janos' review of one-party systems in Eastern Europe between the wars provide valuable historical perspectives on the sources of one-party rule. Jeremy Azrael makes a significant addition to the already large body of literature on the internal dynamics of the Russian party, and James R. Townsend has done a solid and highly original evaluation of the effects of the Cultural Revolution on the Chinese Communist Party. Clement Moore's substantive contribution on the institutionalization of the Tunisian single-party system is a model blending of theory and historical analysis.

The book also contains three impressive and indeed ingenious comparative chapters: Gino Germani on the political socialization of youth in both Italy and Spain, Ergun Özbudun on democratization in Mexico and Turkey, and most unconventional of all, Melvin Croan's imaginative use of Mexico as a model for speculating on the future of East Europe. Although much was once written about the Kuomintang when it was a force on the Mainland of China, Hung-chao Tai has made a distinct contribution by reviewing its current role in modernizing Taiwan.

In an era when in a great variety of situations authorities are beleaguered and governments are increasingly beset with crises of authority, it is useful to be reminded that so much of contemporary history has been dominated by either weak or strong one-party systems. The topic is significant and students of comparative politics will find in this book more challenging empirical propositions than in any book in recent years.

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Political Socialization and Student Activism in Indonesia. By Stephen A. Douglas. (Urbana: University of Illinois Press, 1970. Pp. 228. $7.95.)

The unprecedented intensity of student involvement in politics has generated a growing body of literature concerned with analyzing student political behavior in the United States. But the inherent emotive force and contemporary nature of the phenomenon has made it exceedingly difficult for the political scientist to engage in systematic and long term sttudies of an increasing significant group within the political process.

It is therefore refreshing to note the publication of a book which aptly demonstrates that a basic intellectual dividend may be derived from comparative studies—perspective. Stephen A. Douglas' *Political Socialization and Student Activism in Indonesia* not only provides the reader with an opportunity to examine the role of an emerging political force within a remarkable country, but perhaps more significantly gives us the opportunity to develop insights into the role of a restive domestic student generation. It is of course difficult to draw distinct parallels between the Indonesian student and his American counterpart, but comparisons can be made. In Indonesia, we see the conversion of the outwardly apolitical student population under the period of "Guided Democracy," being transformed into a political force that helped to oust the old regime as symbolized by the fall of President Sukarno in the wake of "The Untung Coup." In contrast, in the United States, the full cycle from political awareness, to political action, to political power has yet to take place. The rise of the Indonesian "Generation of 66" therefore may ultimately represent a model for student activism that may be actualized in this country. An understanding of the course of youthful involvement in the archipelago ultimately not only offers explanations for the conditioners that create activism but may also underscore the problems a student political culture encounters once its initial goals have been realized.

Mr. Douglas' study is the product of an extended investigation. His initial research was conducted in the Republic from 1963–65. In the summer of 1967 he returned to reassess the student political perspective under the Suharto Government.

The author has systematically sampled the attitudes of Indonesian students by engaging in extensive interviewing. In 1964–65 he conducted 116 "usable" interviews "of which 80 were university students and the remainder high school students." "The post-coup interviews obtained in 1967 included 82 university students and no high school students." (p. 22) While one can contend that the author's statistical sample is too limited, it should be recognized that he did not attempt to survey national student opinion but the opinion of the student population of the capitol on "the assumption that Jakarta students effectively control the policy position of the national student movement . . ." (p. 21). The author therefore infers that the elitist character of Indonesian politics is carried over to the realm of student activism. This assumption is questionable especially when one considers the massive degree of mobilization that took place throughout rural Indonesia in the form of an anti-P.K.I. campaign in the period of reaction that followed the "30th of September Movement." It should also be noted that Mr. Douglas has not clearly indicated how many individuals in the 1965 sample were reinterviewed in 1967. While the inherent difficulties in attempting a follow-up study of a rapidly changing student generation is appreciated, the utilization of different samples impinges upon the author's capacity to effectively analyze changes in attitudes. If we assume that the 1965 and 1967 samples reflected the beliefs of two distinct generations, one can accept the comparison. But, many of the apolitical students of 1965 were individually transformed into members of the politicized movement that emerged after the attempted coup.

Mr. Douglas suggests that the changes in attitudes and action on the part of the Jakarta student population were the product of the divergent political socialization process in the Republic. He demonstrates how the various agencies of socialization acted and interacted to create the students' perception of an Indonesian political universe.

In pursuing the study, the author specifically examines how the family, the educational system and the mass media have served to reflect and reinforce the fragmented Indonesian political culture.

Mr. Douglas contends that the "loose" structure of the family and the impact of social change has reduced the capacity of this primary unit to condition political attitudes. Nevertheless, Douglas suggests that the lack of participation by children in the family decision-making process may have helped to inhibit students from entering politics. He also notes that the contradictory role of the father ("ritualized patterns of defense to authority") (p. 35), may help to explain why the younger generation accepted *Pak* (Father) Sukarno, even while they disagreed with his policies. Finally the author suggests that the families preoccupation with controlling and internalizing emotion has led to a wide variety of mental disorders. One can maintain that the massive violence that took place after the coup attempt represented the collective release of deeply held frustrations that could no longer be maintained. The author however is careful not to carry his assumptions too far by noting that "psychoanalytic theory . . . is both especially subject to dispute and especially difficult to test empirically (p. 39).

In his investigation on the role of education, the relationship between agent and action can be more tightly drawn since the author is dealing with the highly organized, specialized and limited institutions of the formal educational process. His conclusion that the *Indoktrinasi* (civics) classes tended to alienate students rather than instill loyalty in the Sukarno regime has direct implications on the role and method of conducting introductory courses on politics in our country which should not be ignored. The charges leveled at civics education in Indonesia bear a strong resemblance to the debates surrounding the development of political science curriculum in the United States.

In completing his study of socialization, Mr. Douglas argues convincingly that it was the mass media which was the prime conditioner to the actualization of student involvement in the post-coup period. Mr. Douglas ably demonstrates how the discontinuity between the call for national unity and the intensive partisan competition that characterized the Jakarta press was not ignored by a student population which read the papers not only as a source of information, but as one of the limited forms of entertainment open to individuals of college age. While there are those who would suggest in contrast that it is the alleged uniformity of television that has stimulated domestic violence, the similarity between the Indonesian and American experience is readily apparent. The mass media in any form is increasingly becoming a leading agent for primary political socialization.

In the final analysis, the parallel between the Indonesian and American experience in student activism is most closely drawn in the author's chapter dealing with the creation of a new student political subculture in the Republic. The author contends that the future of Indonesia may ultimately be stated in terms of the capacity of "the Generation of 66" to channel its newfound identity to meet the enduring problems of a nation in permanent crisis. The activists at the University of Indonesia must now face the challenge of developing a shared national purpose or face the alternative of becoming a fragmented group that can again be readily manipulated by and become an adjunct to the adult political world. At Kent State and Columbia, Drew University or Beloit, an American student population is now facing a similar choice. Will the emergence of a political consciousness degenerate into an empty series of polemics, or will a new awareness create a model of political ideals and action that the political community will have to recognize?

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Sociologists, Economists and Democracy. By BRIAN BARRY. (London: Collier-Macmillan Ltd., 1970. Pp. 202.)

Books which concisely summarize bodies of scholarly work and which provide in addition useful criticism of that work are probably less frequent than is desireable in political science. Brian Barry's book, *Sociologists, Economists and Democracy*, accomplishes both these tasks in truly masterful fashion for limited features of contemporary political analysis stemming from the work of Anthony Downs on the one hand and that of Talcott Parsons on the other. The book is not a general review of all that might be conveniently classified under these origins but rather is a highly selective treatment of the ways in which three political problems are treated in the two traditions. The problems which provide the central organizing scheme are party competition, political participation, and conditions for the maintenance of democratic political systems. This book is must reading for all graduate students, good advanced undergraduates, and all political scientists who care about systematic argument and its relation to systematic evidence.

Barry's analysis of the two traditions is selective. For example, he disposes of Riker's *Theory of Political Coalitions* in a footnote while treating one of Riker's articles more extensively. In the economic tradition there are, in Barry's judgement, two authors of books worth discussing for their theoretical relevance—Downs and Mancur Olson. For authors in the sociological tradition he is somewhat more generous although the inclusion of a larger number of authors is clearly not meant to suggest a higher appraisal of the merit of their work. The joint work of Almond and Verba in the *Civic Culture* is treated rather summarily as is the writing of Harry Eckstein on Norway and democratic stability. S. M. Lipset receives somewhat more space and Parsons himself is given the most extensive discussion. Selectivity in problems and theorists allows selectivity in the systematic evidence employed to help assess the theories. By far the most important source of additional evidence is that drawn from various studies of voting which Barry puts against the theories with insight and telling effect.

The principal virtue of economic theories according to Barry is that they in fact provide deductive structures. Indeed, he sees the deductive structure of economic theories as the feature which primarily distinguishes them from the sociological theories he considers. This distinction, however, undoubtedly rests on the particular choice of sociological tradition and thus by nuance, if not by direct assertion and fair implication, Barry suggests that sociological theories are not deductive. He shows that Parsons is on record essentially to that effect but thus he overemphasizes the extent to which the Parsonian view of the possibilities for deductive theory in the social sciences is shared by scholars reasonably classified as being within that tradition.

The virtue of deductive power does not preserve the economic theorists from criticism. His most important criticism here is the most common which Barry presents as: "Obviously, the constant danger of 'economic' theories is that they can come to 'explain' everything merely by redescribing it. They then fail to be any use in predicting that one thing will happen rather than another." (p. 33) Barry uses this line of argument sparingly but he is continually sensitive to the important consideration that reasonable a priori deductions are necessary if an economic theory is to have any "teeth."

The general plan of the book is to treat specific arguments of specific authors on specific problems. The result is a powerful critique. The inevitable associated problem is that some will cry "foul" and others "totally misunderstood" and still others "see?" None of these responses is appropriate for Barry's critique is a genuine effort to advance the cause of theory relevant to evidence, in short, to our knowledge of politics. To misread the book, i.e., to take it as polemic, would be a serious error but nevertheless one which is likely to be frequent for no author and no work that is seriously discussed escapes often sharp criticism. In spite of the general spreading around of criticism the economic theorists make out somewhat better at Barry's hands than do the sociologists. One of Barry's concluding sentences is revealing in this regard: "The books of Downs and Olson, plus a handful of articles, really do exhaust the field of worthwhile attempts to get from general premises to substantive political conclusions." (pp. 182–183) That is strong language. His concluding criticism of the whole enterprise of empirically oriented theory in political science is that it is woefully underdeveloped with a correspondingly limited literature. Barry's suggested remedy, banal as he notes, is more but better. Given the generally critical tone of the book this is an optimistic conclusion.

One important consequence for readers of Barry's book is that it will probably disabuse them of the noton that economic theories of politics are all deductively rigorous. Futhermore, they will find that much of what is worth knowing about these theories can be presented and discussed in English. It remains true, however, that familiarity with Downs, Olson, and (some) Parsons is a minimum background for reading the book with profit. It is written for those who know the arguments and require only a refresher course rather than for those who seek a "pony" on contemporary empirically oriented political theory.

The book is written with grace and wit. It is evident that Barry reads carefully (by implication he suggests some scholars do not) and it is equally clear that Barry is as capable of close argument as the best of the theorists he critiques. An additional virtue of the work is that it is anchored in history. Although very little space is devoted to historical roots, enough is said to place Barry's analysis, as well as the sociological and economic theorists, in a tradition. This book is short, perhaps too short, but it is meaty and it deserves to be (very carefully) read by political scientists.

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The Intelligence of Democracy: Decision Making Through Mutual Adjustment. By Charles E. Lindblom. (New York: The Free Press, 1965. Pp. 335. $7.95.)

The Policy-Making Process. By Charles E. Lindblom. (Englewood Cliffs: Prentice-Hall, Inc., 1968. Pp. 118. $2.25.)

Professor Lindblom has written these books to fulfill different functions. *The Intelligence of Democracy* is a theoretical monograph which builds upon the exposition of decision-making as a strategy of disjointed incrementalism developed by Lindblom and Braybrooke in *A Strategy of Decision.*[1] *The Policy-Making Process* is an introductory text in which Lindblom outlines the institutional behavior patterns and analytic tools which characterize "the play of power" in the policy-making process. *The Policy-Making Process* is not a new stage in Lindblom's evolving argument, but rather a summary of the empirical and normative conclusions he and his collaborators have reached in earlier work. Therefore, we will concentrate upon *The Intelligence*

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[1] D. Braybrooke and C. E. Lindblom, *A Strategy of Decision* (New York, Free Press, 1963).

of Democracy and its contribution to the corpus of Lindblom's work.

Lindblom's broad objective in this book is "to undertake a systematic comparative analysis of centrality and partisan mutual adjustment among various kinds of political officials and leaders as competing methods for rational coordination of governmental decisons . . ." (p. 9). Lindblom defines coordination of a set of decisions as adjustments through which "adverse consequences of any one decision for other decisions in the set are to a degree and in some frequency avoided, reduced, counterbalanced, or outweighed" (p. 154). Lindblom defines centrality as a process through which one decision-maker, in a set of decision-makers, has a symmetric, dominating, control relationship with every other member of the set, and who uses this relationship to adapt, or coordinate, the decisions of that set (p. 105). He defines partisan mutual adjustment as the various adaptive or manipulated processes through which interdependent, partisan decision-makers can coordinate with each other "without anyone's coordinating them, without a dominant common purpose, and without rules that fully prescribe their relations to each other" (p. 3). Partisan mutual adjustment produces coordination as a byproduct of ordinary decisions, not as a specialized subset of "coordinating" decisions through a central coordinating mechanism. Thus, for partisan mutual adjustment, the study of coordinating devices becomes coextensive with the study of policy-making itself (p. 9).

Such policy-making through partisan mutual adjustment is characterized by the absence of cooperation among decision makers: by the absence of common objectives, agreed-upon application of values to concrete decisions, or agreed-upon criteria to govern mutual adjustments (pp. 28–9, 106, 132). Instead, partisan mutual adjustment is the set of processes through which *partisan* policy-makers advocate and pursue their specialized interests according to a strategy of disjointed incrementalism, the policy-making strategy Lindblom developed in earlier works.[2] Values which an advocate for any given interest neglects are typically "mopped up" by advocates of the neglected values. Lindblom's empirical conclusion is that partisan mutual adjustment pursued through the strategy of disjointed incrementalism characterizes the policy-making behavior of all major political institutions in democratic, polyarchal systems in situations of complex policy choice (Chap. 6 and pp. 151, 160–1).

Lindblom's formulations of the processes of partisan mutual adjustment and the strategy of disjointed incrementalism have, under the collective term incrementalism, become a major benchmark in the policy sciences. Incrementalism constitutes an interrelated set of empirical hypotheses about the role structures and rules of the game through which policy-makers perceive their environment and pursue their policy goals. Most policy scientists would agree that many policy-makers do in fact operate through such processes and according to such strategies, and that Lindblom has made a significant contribution in developing an empirical theory of great explanatory power.

The evaluative, or normative implications of Lindblom's work are more controversial. Lindblom treats incrementalism as one polar conceptualization of the policy-making process, in contrast with various central-coordinating, rational-comprehensive, or synoptic techniques such as planning-programming, budgeting or systems analysis.[3] What is at issue between Lindblom and the critics of incrementalism are his treatment of these two conceptualizations as polar and largely irreconcilable, and his conclusion that incrementalism is likely to produce better decisions than does synoptic decision-making.

Lindblom moves towards this evaluative conclusion in two stages. First, he argues that in situations of complex policy choice, synoptic problem-solving is impossible, given the underlying heterogeneity of value preferences (pp. 168, 286). Intellectual capabilities, information, and monetary resources are all insufficient for the task. Values are either conflicting or unknown. Finally, a definite policy solution is unlikely due to the interrelationship of fact and value, the open-endedness of values and policy alternatives in a technologically dynamic society, and the ongoing nature of policy disputes (pp. 138–149). Thus, Lindblom concludes that since synoptic policy analysis is empirically impossible, it is also normatively irrelevant in situations of complex policy choice. If synoptic, central coordination cannot be used to resolve policy choices, it cannot be as good as the formalized "pulling and hauling" of policy-making through partisan mutual adjustment.

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[2] According to the strategy of disjointed incrementalism, policy-makers compare and evaluate only increments of policies at the margin. They consider a restricted number of policy alternatives and consequences for any given policy alternative. They engage in constant readjustment of ends and means, returning frequently, or serially, to the same problem. Finally, they concentrate on remedial changes in social ills rather than on the pursuit of a well-defined future state (pp. 143–8).

[3] The synoptic method, by Lindblom's definition, is a comprehensive method of decision-making which requires: identification and ordering of the objectives and values being sought; a comprehensive survey of alternative means of achieving those values; an exhausive examination of the consequences of each alternative; and, finally, the choice of an alternative policy that will achieve a maximum, or at least an acceptable amount of the values sought (pp. 137–8).

Having made this negative case against synoptic analysis, Lindblom then suggests certain positive attributes of incrementalism which promote "calculated, reasonable, rational, intelligent, wise—the exact term does not matter—policy making" (p. 294).

First, conflicting values in society are protected through partisan mutual adjustments by their advocates, so that no consequence adverse to one value will be ignored. In addition, the large number of advocates provides the policy-making process with energy, intelligence and skill, ensuring a thorough clarification and defense of relevant values. Thus, any value advocated by even a small group will be weighted in the policy-making process. Moreover, the more widely and intensely held the value, the greater the weight it will receive. Such representation of values through partisan mutual adjustment is in contrast to synoptic analysis which cannot exhaustively review all relevant values, thereby necessarily ignoring some (pp. 140, 151).

Second, partisan mutual adjustment promotes agreement on both values and policy since partisan policy-makers are highly motivated to achieve their own values. Hence they are willing to accept moderate, open-ended decisions. Such compromises in turn actually form new, widely shared values, reconciling previously conflicting interests through persuasion, alliance-building, bargaining, compensation and the like (p. 206).

Thus, Lindblom argues that incrementalism, unlike synoptic analysis, successfully maximizes the skills and information available to the policy-making process, reconciles values, and arrives at policy decisions, albeit tentative and ongoing ones. In summary,

> the synoptic idea of problem solving is fairly straightforward. To solve a problem one must first understand it—one masters it. In contrast, behind the incremental and disjointed tactices . . . is a concept of problem solving as a strategy. In this view, public policy problems are too complex to be well understood, too complex to be mastered. One develops a strategy to cope with problems, not to solve them (p. 148).

It appears to this reviewer that Lindblom is evaluating incrementalism and synoptic analysis according to different standards. Most policy scientists would agree that synoptic analysis cannot "solve" complex policy choices in any final way, just as, by definition, incrementalism cannot. Any suggestion that synoptic analysis can "solve" such choices is tantamount to a "straw man" argument. Important social choices at a high level of value aggregation must be made through political processes which resemble partisan mutual adjustment and the strategy of disjointed incrementalism.[4]

Synoptic analysis can, however, serve as an adjunct to incrementalism and, in important ways, improve the ability of policy-makers to cope with problems. In comparing partisan mutual adjustment and PPB, for example, Charles Schultze has remarked:

> No one conceives of PPB as a system that makes decisions. Rather, it is a means of helping responsible officials make decisions. It is not a mechanical substitute for the good judgement, political wisdom, and the leadership of those officials. The question at issue is *not* how the PPB can supplant the political process but whether and how it can fit into that process.[5]

Lindblom, in concluding that synoptic analysis cannot solve problems but that incrementalism can cope with problems unnecessarily accepts some important disadvantages of incrementalism and fails to recognize important advantages of synoptic analysis in coping with complex policy choice. The critics of incrementalism argue, on the other hand, that in certain cases biases inherent in incrementalism can be compensated for by systematic analysis. We shall conclude this review by citing four such cases.

First, we cannot assume that the advocacy process underlying incrementalism always brings out the information about program inputs and outputs necessary to an informed decision. As Charles Schultze has convincingly argued, contemporary policy-making is often so technically complex that decision-makers cannot directly and/or intuitively evaluate program proposals in terms of political values. Program inputs in such technically complex policy areas must be translated into policy outputs before the values they serve can be evaluated.[6]

Second, incrementalism may be subject to organizational bias. Lindblom states that if incrementalism is to operate in a normatively acceptable fashion, special social conditions must obtain. There must be many partisan decision-makers with a great variety of attitudes and interests, so that no important social value is neglected (p. 151). Lindblom enjoins us not to confuse the processes of partisan mutual adjustment generally with a particular distribution of weights among specific interests in any given process of partisan mutual adjustment (p. 245). Further, Lindblom suggests that the appropriate corrective for a biased distribution of weights in the policy-making process is an alternate distribution of power among the participants.

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[4] C. L. Schultze, *The Politics and Economics of Public Spending* (Washington, D.C., The Brookings Institution, 1968), pp. 74–5.

[5] *Ibid.*, p. 17.

[6] *Ibid.*, pp. 63–4.

There are, however, frequently situations in which the structure of interests permits only limited values to be represented and constitutes an ongoing organizational bias against alternate distributions of power. Since specialized bureaucracies consistently initiate policy, and unorganized, apolitical, or poor interests consistently lack well-established access to the policy-making process, such organizational bias frequently prevails and excludes the consideration of certain political values and policy alternatives. In such cases, incrementalism may consistently serve interests closely related to the status quo, and consider only proposals for moderate change moving towards a Pareto optimum rather than radical changes which may require redistribution of values. Furthermore, due to differential access, the corrective Lindblom advocates may well be impossible in the short-run and extremely costly and difficult in the longer run.

Third, Lindblom argues that remedial and serial policy-making can cope with emerging problems.

> In short, if decision making is remedial and serial, anticipated adverse consequences of any given policy can often be better dealt with if regarded as new and separate problems than if regarded as aspects of an existing problem. And unanticipated adverse consequences can often be better guarded against by waiting for their emergence than by often futile attempts to anticipate every contingency as required in synoptic problem solving (pp. 150–1).

There are cases, however, in which such serial and remedial decision-making might cumulatively skew the policy output. Synoptic analysis might, on the other hand, produce different policy outputs and thus be an appropriate aid to rational decision-making.

To cite only one example, consider the extremely specialized policy process which determined the escalation of US involvement in Vietnam in 1964–1965. Adam Yarmolinsky has observed the disadvantages connected with the remedial, serial incremental counterforce policy which was pursued in this period compared with the putative advantages of a more synoptic analysis.

> It is easy to justify an escalating intervention by pointing out that you are only matching force with force and greater force with greater force. . . . In each case the use of force to counter force may be justifiable in reciprocal terms, as seen by the person initiating the counterforce, but in human terms it may only result in avoidable tragedy. . . . If the question is asked whether previous events justify the use of force in response, one may be tempted to reply in the affirmative; but if the question is put, rather, as to whether the alternative involving the use of force is the best one available in light of all the facts and circumstances, the answer may be quite different. And if the question of alternatives is raised, not only at the outset of a course of action involving the use of force, but at every stage of potential escalation as well, it may result in a process of involvement that will indeed be more cautious and more controlled than some of our recent involvements in other countries' problems.[7]

The incremental analysis of disaggregated aspects of emerging problems may, in other words, cumulatively create a policy which might be rejected if, at any stage of the policy-making process, that output were systematically compared with alternative policies.

Fourth, in some cases synoptic analysis may produce more and/or better values than incrementalism, in contrast to Lindblom's argument that incrementalism typically produces normatively good, consensual values. In the 1954 RAND study of overseas bases, for example, systematic analysis "discovered" a new value for which there had been no initial support in the advocacy process. The study originated as a logistics exercise to determine an efficient method of basing and fueling the strategic bombing force. It became apparent early in the analysis, however, that the available policy alternatives were extremely sensitive to the order of nuclear strike: that is, all were significantly less effective if the USSR struck first. Therefore, the objective of the analysis was transmuted into that of reducing the vulnerability of SAC to a Soviet first strike. The analysis, in effect, "discovered" the new value of strategic invulnerability.[8] In policy areas characterized by rapid technological change, such as nuclear weaponry or space, such synoptic analysis may be particularly needed to compensate for the status quo bias of incremental policy alternatives.

In conclusion, the critics of incrementalism are concerned that as social scientists, we do not view a policy as normatively acceptable simply because the processes of incrementalism have arrived at it. According to Lindblom, such a consensual policy *ipso facto* constitutes a normatively good policy since it has mustered widespread agreement and is presumably a Pareto Optimum. Alternatively, the critics argue that although consensus may be a necessary condition of good policy, it surely is not a sufficient condition.

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[7] R. M. Pfeffer (ed.). *No More Vietnam?* (New York, Harper and Row, 1968), pp. 107–8.

[8] See E. S. Quade (ed.), *Analysis for Military Decisions* (Chicago, Rand McNally and Co., 1964), Chap. 3.

Moreover, the critics of incrementalism are concerned with pushing out the limits of effective systematic analysis rather than with emphasizing, as does Lindblom, its limitations. In an admittedly complex universe, the systematic analyst as well as the incrementalist recognizes that the universe is too complex to completely understand. The systematic analyst, however, wants to give up later rather than sooner.[9]

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[9] See Schulze, *op. cit.*, p. 37. But see C. F. Lindblom, *The Policy-Making Process* (Englewood Cliffs, N.J., Prentice-Hall, Inc., 1968), p. 27.

BOOK NOTES

POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

Law, Society, and Industrial Justice. BY PHILIP SELZNICK WITH THE COLLABORATION OF PHILIPPE NONET AND HOWARD M. VOLLMER. (New York: Russell Sage Foundation, 1969. Pp. 282. $7.00.)

This is a contribution, brilliant and substantial, to the literature on private government. Specifically it deals with employee disciplinary and grievance procedures. (Selznick is a sociologist interested in the sociology of law and of law-touched phenomena.)

The book reports some empirical studies of employee attitudes, presents a sociological analysis of personnel government, and surveys much of the relevant social science and legal literature. But its core lies in its theoretical chapters.

Analysis begins with the evolution of the legal ideas, and of what the authors call moral ideas, relevant to the subject. "'Moral evolution' is a somewhat unfashionable phrase, but it can usefully denote the progressive clarification of human ideals, and the enlargement of institutional competence to serve them. Our concern is with a special ideal—the rule of law—and its extension to the conditions of employment in modern industry." P. 1. The context is "the embodiment of ideals in institutions, the infusion of group life with the aspirations and constraints of a moral order." P. 1. The law of the work-place is a strategic point at which to make inquiry.

Law to the authors is generic, not uniquely associated with the state; its gist is not simply coercion but social control through formal authority and rule-making. Further, law properly conceived contains an ideal element; it is a realm of value, and we rightly "distinguish a developed legal order from a system of subordination to naked power." P. 11. It and legality root in rationality, in the progressive reduction of arbitrariness. Still further, legality, as rationality and reduction of arbitrariness, has affinities with the ideal of political democracy in treatment of the civic participant, in emphasis on constraints on power, and emphasis on participation.

As to moral evolution, the authors see, from Durkheim, Weber, Piaget and others, a tendency toward the possible development of a morality of cooperation—of linked specialization, so to speak—in evolving societies as distinguished from more primitive or traditional moralities of constraint. This in turn fosters rationality.

The two, legality and the morality of cooperation, are emergent social phenomena neither accidental nor matters of subjective preference, and are strongly congruent with each other, the second playing a large role in the development of the first. But legality, under the impulse of the two moralities and a developing society, is not a static affair of rules. Rather it is an affair of evolving doctrines. And the line between the legal and the political is progressively blurred as its ideals are absorbed into the political order. In the end law merges into polity, as the criteria of legitimacy extend and deepen.

There is of course nothing inevitable about this evolution. There are latent values, as seen by normative theories of law. But there are also the insights of legal realism and sociological jurisprudence; law is also a becoming, driven by needs and conditioned by circumstances.

To bring all this to bear on private government requires a law of associations. "To extend the rule of law is to build it firmly into the life of society, to make the master ideal of legality a true governor of official conduct. If this is to come about, political and legal theory must lend a hand. It can do so by fashioning concepts and doctrines to bridge the gap between new social realities and the received legal tradition." P. 35. This is the next problem.

Here the doctrinal (less so the conceptual) resources of law are rich; the authors' contribution, a large one, is rethinking and reorganizing. It is in two stages, a critical one early in the book and a creative one at the end.

In the first they deal with these resources, with the values and weaknesses of pluralist thought as a means of rationalizing the legal materials, and with the sociological character of the institutions that generate private government. A crucial assertion is that we may want to see the institution "as a potential body politic, as a legal order struggling to be born." P. 52. There is a distance to go. Classic views of the corporation are inadequate as theory and as pictures of reality. There are tensions between the premises of the law of contract and an adequate treatment of association. So also for the law of property, another possibility. A reconstruction of the whole theory of organizational authority, and of the association's entire inner order, in the light of sound public policy is required. This the authors essay in their final chapter, "Private Groups and the Law of Governance."

They pose the problem as that of how to extend, *mutatis mutandis*, to private organizations the ideals and techniques available in public law for doing justice without destroying workability.

They begin by decisively undermining the distinction, sharpened by nineteenth century liberalism's separation of state and society, between private and public law; when they are done, the resources of public law are available for extending legality to private government, a central analytic tool being enlargement of the meanings of citizenship and civic participation under the sign of the theory of emergent polity.

This done, the concept, broadly perceived, of due process as the positive law of legality, as the law of governance in general, comes to bear. "Thus, as a public-law idea giving definite form and substance to legality . . . due process may help us find a way to extend the rule of law to areas hitherto controlled only by concepts of private law." P. 250.

But if there is such a law of governance to what should it be applied? The authors' answer is broad: whenever the nature of the association and its members' relationships to it create, or in justice or practicality require to be created, the private equivalent of citizenship.—Winston M. Fisk, *Claremont Men's College.*

The Active Society: A Theory of Societal and Political Processes. By Amitai Etzioni. (New York: The Free Press, 1968. Pp. 671.)

Alvin Gouldner, in his recent book *The Coming Crisis of Western Sociology* (Basic Books, 1970), argues that "the central locus of the tension in functional theory is in its ways of dealing, or not dealing, with social change. Functionalism's drift toward a convergence with Marxism is an effort to cope with the tensions it feels in this intellectual area, as well as an indication of the mounting crisis it is undergoing." Amitai Etzioni's *The Active Society* is a major work marking the leftward drift of American sociology and like Ralf Dahrendorf's *Class and Class Conflict in Industrial Society,* also part of the drift toward Marxism, it has been vigorously attacked by the Marxists and by the declining functional school as well.

The active society is a future state, the detailed definition of which provides the normative and transcendental basis of an approach to social problems which Etzioni labels "societal guidance." As Etzioni indicates in a companion text (*Societal Guidance,* Crowell, 1969), societal guidance is a theoretical framework for societal efforts to treat social problems. As such it is opposed to sociologies which revolve around questions of social origins, genetic patterns, or functional consequences. It may also be contrasted with aggregative approaches to social order, such as those embodied in the economic full competition model or the veto group analysis of national politics. It represents a departure from homeostatic systems analyses and from various voluntaristic conceptions of history, as embodied in rationalistic game theory or implied in Marxism-Leninism. Political scientists will find its critique of pluralistic decision-making analysis of the Dahl and Lindblom varieties equally important challenges to prevailing doctrine.

It is from the explicit opposition of societal guidance to these alternative methodologies that Etzioni derives his subtitle, "A Theory of Societal and Political Processes" and his claim to advance foundations for a new theory of macroscopic action. That the scholastic jargon in which Etzioni indulges at length separates the reader from the American experience, as one critic has charged, should not obscure the importance of the attempt to define a consistent theory of political process intermediate between functionalism and Marxism.

No review can do justice to the intricate argument that Etzioni has constructed at enormously greater length. In general, however, Etzioni argues that in the modern and "post-modern" (an unfortunate term) phase, an active orientation, marked by self-knowledge, commitment, and access to power, is developing and that just as the first social revolution led to corporate organization, so a second social revolution will generate a higher level of social guidance organizations, characterized by high degree of control over social assets and power and by high responsiveness to basic human needs: the active society.

Etzioni discusses how types of action may be differentiated by the degree to which they are subject to control processes, the basic elements of which are knowledge, commitment, and power (in turn divided into violent-coercive, material-utilitarian, and symbolic-normative types). Power and societal control, while rooted in the stratification system, involve man's capacity for symbolization and transcendance (pulling oneself into a projected future). In this sense Etzioni sees a basic unity between idealism and materialism.

Etzioni then goes on to critique approaches which seek to explain the variance in social phenomena on the basis of subsocietal units like individual roles or the behavior of small groups. This is followed by a lengthy criticism of the aggregative theories, such as balance of power models of international relations and other approaches interpreting the societal state as the sum of individual actors; collectivist or system approaches, which interpret the societal state as a gestalt; voluntaristic approaches, which assume that man can restructure society. Etzioni then sets forth an alternative, the theory of societal guidance, integrating collectivist and voluntaristic elements. The language of social guidance analysis focuses on knowledge, commitment, and power as bases of capability for societal choice and planning. Etzioni emphasizes the connection of this approach to cybernetic emphasis on knowledge, the dynamic nature of the analysis, and its emphasis on functional equivalents.

After setting forth the societal guidance approach, the collectivity is established as the basic unit of macroscopic action, in opposition to the role or the organization. The collectivity, based in the stratification system, derives its cohesion from a leadership net primarily, and from shared norms only secondarily. The state, which has power in its own right, does not merely reflect normative consensus and is more an agent of control than of consensus-formation, has a pyramidal pattern capped by a state-controlling elite (defined by role rather than class). The state as an effective control and guidance network depends on the nature of its relation to societal collectivities.

The functional requisites of a fully active society include knowledge (here Etzioni discusses the capacity of elites to give contextuating orientations), power (discusses the need for intellectual and expert filters to political decisions of state elites), and commitment (rejects the Lindblom incrementalist model in favor of a mixed-scanning model). Moving to discuss the implementation of decisions, Etzioni defines power as a generalized capacity, not inferable from a single case decision, which must be operationalized by inference from social assets.

Power is thus linked to a stratification (not necessarily class) base. The state, wishing to implement decisions, undertakes mobilization (collectivization of energy, aggregating assets at disposal of the state), but this in itself is insufficient for social support—responsiveness, primarily to organizations representing collectivities in turn rooted in the stratification system, is also essential. To make the transition from a "drifting" sociey (high in consensus but low in guidance) to an "active" society (high in both), certain structural changes are necessary to enable inter-collectivity consensus: centralization (less states' rights), access to control centers (minority inclusion), regulation of the economy, "inter-woven" planning (a conducive mix of state ownership, cooperative, private firms, etc.).

Thus the trend is for business influence to decline and for control systems to emphasize normative and utilitarian rather than coercive sanctions, since this is less alienating and hence more effective. The lever here is the willingness of the more powerful collectivities to allocate political power to the weaker in the hope that this will drain their drive for redistribution of assets. This mobilization of weaker collectivities is key to the transformation of monopolized societies into more active ones. This movement from an "inauthentic" society (claims responsiveness, but is alienating) to an active society may be measured by the extent to which specific basic human needs are met (for affection, recognition, repeated gratification, stability, etc.). The member of an inauthentic society is forced to choose between retreatism and activism.

Ultimately, *The Active Society* remains at the level of taxonomic sociology, not attempting to anchor social change in a phenomena with a describable natural history which need not be explained itself (as in Marx and Freud). While beginning to reacquaint functionalism with Marxist emphases on alienation, power, and stratification, all Etzioni means by theory is "a process in which concrete data are 'broken down' into abstract components and reintegrated on still more abstract levels" (p. 42). To refocus on a more concrete level useful to political analysis, it will be necessary to further specify the manner in which power, collectivities, and hence the state are rooted in stratification systems, and to articulate more clearly the manner in which less powerful collectivities are mobilized to restructure monopolized societies. These theoretic needs represent the forces behind what Gouldner labels the "drift" of functionalism toward convergence with Marxism.—G. DAVID GARSON, *Tufts University.*

Exit, Voice, and Loyalty: Response to Decline in Firms, Organizations, and States. BY ALBERT O. HIRSCHMAN, (Cambridge: Harvard University Press, 1970. Pp. 162. $6.95.)

The problem analyzed in *Exit, Voice, and Loyalty* is the deterioration of performance in economic and political organizations. The quality of performance by firms in the economy can decline, and in an analogous manner, the performance by political parties can create dissatisfaction with its membership. As another contemporary work in political economy the author seeks first to convince the reader that certain problems which exist in the economy exist also in the polity, and then to suggest that the same concepts can explicate both problems.

According to Hirschman, the solution of the problem of quality deterioration is an effective use of either an "exit" option or a "voice" option. Individuals who receive benefits from the products, policies, or services of an organization, whose quality of performance is decreasing, can either reveal their dissatisfaction by leaving the organization and searching for another one or by verbally protesting their views to the leadership. The extent to which individuals use either option depends upon availability and cost of the options, and individuals' "loyalty" to the organization. Loyalty or identification with an organization tends especially to decrease the use of the exit option.

In his concluding remarks, the author claims that there is no specifiable optimal combination of voice and exit that can arrest deterioration in organizational performance (pp. 124–5). Without contradicting that claim, the author seeks to persuade the reader that voice is an essential mechanism for terminating deterioration in the polity. Since political scientists should primarily be interested in the author's discussion of the polity, it is important to focus upon his analysis of voice. There are at least five claims in his argument.

First, the author notes that economists overemphasize the efficacy of the exit option (pp. 15–19). He suggests that while exit is the primary alternative in the economy it is not the only one available in the polity. Second, in order for voice to be effective there must be a combination of actors in the polity. Some actors must always be alert to deterioration while others need to utilize voice only when their essential needs are threatened (pp. 31–32.) If every individual would use the voice option constantly, then there would be an overreaction to the decline in performance and the lapse would not be corrected (p. 31).

Third, he argues that the members of the polity who would be most effective with voice, i.e., those who can articulate dissatisfaction, are those who will first try to use the exit option (pp. 47, 50–1). He offers the illustration of parents' reactions to the decline in the quality of public schools and asserts that those who could most effectively voice opposition to the school administration would be the first to enroll their children in private schools. He states that those individuals "who care *most* about the quality of the product and who, therefore, are those who would be the most active, reliable, and creative agents of voice are for that very reason also those who are likely to exit first in case of deterioration" (p. 47). The fourth point in the author's argument is that the most effective use of voice can be made when the exit option is not available due to high costs of exiting or induced loyalty (pp. 92–3). He offers a critique of the Harold Hotelling-Anthony Downs model of political party ideology to demonstrate this point. He argues that the Hotelling-Downs model cannot predict party positions, such as the shift to the right by the GOP in 1964, because it does not consider the effectiveness of voice by individuals with extreme ideological positions. If the voters on either extreme of a liberal-conservative continuum have "no where to go," they will be highly motivated to voice their views and influence party leadership. Unlike moderate party members the extremists will have high levels of participation. In this manner, the GOP in 1964 followed the author's argument and falsified the prediction of the Hotelling-Downs model.

Finally, the author believes that when the exit option is used in the polity it is improperly used. He claims that since the polity deals with public and not private goods as in the economy, the exit should be slightly different for each activity. When an organization provides only private goods the members who exit will not criticize the organization since they are not affected by it as non-members. However, with public goods, both members and non-members can be affected by the policy of the organization. Hence, when individuals exit from political organizations they should publicly criticize any policies that they oppose. On this basis, the author criticizes officials in the Johnson administration who disagreed with the Vietnam policy, but who would only give private reasons for exiting. This "malaise" the author believes was countered by Senator Eugene McCarthy's public dispute over the Vietnam conflict (pp. 103–5).

Of the literature in political economy published since Arrow's *Social Choice and Individual Values,* this work is one of the most broad-gauged arguments and one of the most politicized. However, there are some critical questions that can be raised about it. First, what is the range of political organizations that are subject to a decline in the quality of performance? Does it affect interest groups as well as parties? Does it affect legislatures as well as bureaucracies? Second, what criterion specifies the "decline in the quality of performance"? Can the decline in quality be defined independently of behavior expressing dissatisfaction? This is an important aspect of the work because the author assumes that concern for the quality of the schools, neighborhoods, or public policies can be identified and measured. Second, he assumes that concern for service is highly correlated with the ability to effectively voice dissatisfaction. Finally, he assumes that activity, reliability, and creativity of voice can be identified in a measurable manner. Third, what is the connection between the quality of services and the benefits of the services? Do actors prefer a certain combination of quality and benefits? Fourth, are there some actors whose rôle precludes them from even considering exiting or voicing dissatisfaction? Do some actors make a decision, not on the basis of loyalty, to always accept the quality of performance? Finally, under what conditions will individuals choose to exit or to voice dissatisfaction? The author mentions that costs are associated with each alternative, but he fails to specify precisely when these costs are high or low. If the author can answer these questions in future research, then *Exit, Voice, and Loyalty* will lead to some of the more interesting work in political economy.—ROGER A. HANSON, *University of Georgia.*

Open Systems: Arenas For Political Action. BY HENRY S. KARIEL. (Itasca, Illinois: F. E. Peacock Publishers, 1969. Pp. 142. $3.50.)

Open Systems refers to Henry S. Kariel's belief that we are presently faced, in our personal, social, and political worlds, with what he terms "closures," to which the proper response is greater openness. In our personal lives, closures are manifested by rigid conceptions of role definition; in our social and political lives, by rigid institutional boundaries that prevent adequate response to the shifting realities of contemporary life. Furthermore, our very language itself, particularly that of academic social science, joins in and helps encapsulate us in our closures by "bracketing out" all too many aspects of reality, especially those threatening established political and social elites.

Kariel is convincing in his central assertions. The book is provocative and at times even exciting for any frustrated middle-class academic wondering in the words of political theorist Peggy Lee, "Is That All There Is?" It is this very appeal, however, that also signifies the volume's greatest failing. Kariel admits early in the book that he is speaking to a special audience. Recognizing that, "As I take time out to form my sentences, men elsewhere are being used, violated, discarded," he goes on to say, "My words are not directed to them. Nor are they directed to their wholly preoccupied rulers—specialists in violence who *know* what is just and are very busy doing justice. I can only expect to reach those in the middle who are fortunate enough to share my shelter, who are not fully involved oppressing others or fighting oppression, who can afford to read and listen, to think, and to talk back." (p. 10.) His very definition of politics, moreover, is linked to his specialized constituency. The "specifically political quality of society," he tells us, is "its playfulness." (p. 6.) Politics at its best, therefore, is not instrumentally linked to the attainment of goals, but is rather the sheer joy of interaction with others for its own sake.

It is an irony befitting Karel himself, who preaches the virtues of an ironic stance toward life, that he has recently been the victim of a slanderous attack within the American Political Science Association for his membership in the Caucus for a New Political Science, presumably because of the Caucus' alleged linkage with the contemporary radical left; for Kariel's vision of politics is aristocratic to the core, speaking to and having relevance for only the relatively well-off in what he himself recognizes to be a society featuring gross inequalities in access to the various goods of life. Nietzsche really is the patron saint of Kariel's vision of openness; Kariel in no way shares Nietzsche's contempt for the mass (he is too decent a liberal to do that), but the problems of the overwhelming majority of men for whom politics is not play but rather a desperate attempt to achieve the minimal conditions of a humane life, have little emphasis in his argument.

This is not to say, however, that Kariel's argument is thus trivial. He joins someone like Robert J. Lifton in exploring the potential life-style of what Lifton calls "Protean Man." If Kariel has nothing to say about the black or poor white, he has much to say about the dilemma of those large numbers of Americans who have "made it," and then, like J. S. Mill, discovered that the conventional notion of happiness or fulfillment was insufficient. When Kariel says, for example, "that indeed we should ultimately accept as democratic nothing less than a society all of whose members are active participants in an interminable process —*and who will not mind such activity,*" (p. 73, italics in original) he is saying something with very radical implications for a variety of contemporary American institutions. If indeed all the world's a stage, then those of us who are not fullfledged players in it are therefore being deprived of crucial aspects of human existence. Whether the institution in question is the business corporation, the university, or the American Political Science Association, none offers remotely enough "parts" (as distinguished from the all-too-ubiquitous "roles") so that everyone can participate in the drama of political life. (Perhaps then the defenders of the *status quo* were wise to attack Kariel, for in a bureaucratic society based on rigidity, a Nietzschean aristocrat is perhaps as dangerous as a more traditionally left-wing radical.)

Is the book worth reading? My answer is a qualified yes. Kariel does not speak to *all* of our political problems, or even to what many of us might believe to be the most important ones. On the other hand, he reminds us of the importance of utopian and genuinely speculative thought, because of both the relevance to our own middle-class lives and the wider question of conceiving the good life, even for the presently oppressed, in terms beyond the vision of the welfare state. And, ultimately, the chief virtue of his book might rest in what a more conventional approach would see as a defect. The volume is not so much a systematic argument to which a reader (or reviewer) can respond on a formal level, but rather a series of poses, of possibilities, asking us to test them out as parts of our own lives and realities. In the less than two hours that it takes to read the book we are jarred into new ways of conceiving of ourselves as political scientists, as citizens, and ultimately as human beings.—SANFORD LEVINSON, *The Ohio State University.*

Value Judgment and Social Science. BY EUGENE J. MEEHAN. (Homewood. The Dorsey Press, Inc., 1969. Pp. 159. $2.50, paper.)

Eugene Meehan's, *Value Judgement and Social Science,* is a useful and provocative book, which, as the author says in his introduction, was his major intention when he wrote it. In that sense he has fulfilled his purpose admirably. It is useful because it addresses social scientists generally, and political scientists particularly, concerning the cognitive processes and analytic structures involved in that all too human activity—moral evaluation. To be sure we have libraries of works devoted to how we ought to behave, but precious few to how we ought to think about how we ought to behave. This is one of the latter.

It is provocative because it helps to cut the ground from under at least three different popular or once popular stances with respect to the relation between description and explanation on the one hand and evaluation on the other. The first I can only call (with the author) a kind of mindless empiricism. It holds that social science deals only with Facts immediately and plainly available in reality, and eschews any sort of value judgement on the grounds that they can only be private and subjective responses. This view has been known to be scientifically untenable for at least twenty years, although Meehan, properly I think, repeats the point that scientific explanation deals not with external reality but our preceptions of it. And he goes on to add that no human ever actually acted according to this description, for action involves choice and choice involves evaluation. It is existentially impossible to avoid value judgements and therefore disastrous to attempt it.

The second position which Meehan takes on is advanced by those who, in reaction to the kind of raw empiricism mentioned above, seek to turn social science into a body of moral aphorisms derived variously from the great works of the past or from relevant social critics of the day. The difficulty here, says Meehan, is that moral choice is not and cannot be made independent of the world in which men live. Evaluation demands adequate description and explanation in order to make real choices. Adequate evaluation in the social sciences therefore demands a sharpening and extension of the methodological tools of empirical social science, not their abandonment. In fact it is Meehan's point that the acceptance of the positivist interpretation of the fact—value dichotomy has done more harm to the evaluative enterprise than anywhere else because it allows those making such judgements to escape the methodological and analytic rigor normally demanded of those who deal with other kinds of concepts.

The third position which Meehan attacks is one not perhaps so prevalent in academic circles but certainly present as a trend in contemporary social criticism. It is the view that moral choice can and must be separated from criteria of rational thought—indeed from any criteria. Such a view is at least implicit in the current (following a long line) cult of immediate sensation—the hippies. But, says Meehan, to act without criteria for choice is neither moral nor rational. The opposite of rationality is not morality but randomness. The author pleads powerfully that ethical concepts, like other concepts, are constructs of the human brain. As such their quality can and should be judged by the same standards; namely, the quality of the reasoning that lies behind them.

One can fault this book at points. The generalizations are sometimes sweeping, due perhaps to the style of writing more than anything else. Some claims are grandiose. Consider, for example, "In very general terms the approach to inquiry accepted here combines empiricism, naturalism, instrumentalism and pragmatism, but without dogmatism." (p. 12) This reader's reaction was that we would be doing well indeed to get all that packed in 155 pages. And finally the author is sometimes careless in his own use of language. In an introductory discussion of the relation between fact and value Meehan says:

> Granting that a value judgment is not a statement about "reality," and that it cannot be "proved" by reference to factual data . . . it does not follow that an explanation *is* such a statement. While descriptions, explanations, and evaluations relate to empirical observations, they cannot in either case be deduced from observations, nor are they "discovered" in the external world. (pp. 5–6)

But one can assert that, say, an explanation has been "proved" in the sense that it comports with all known observations without at the same time asserting that it is either deduced from those observations or discovered in the external world. The point the author is making here is a very good one. My impression is that he might have made it somewhat more carefully.

But the above are minor, some might say picayune, complaints. For those interested in the topics raised here, and few of us are not, Eugene Meehan has written a very good book. He has also written one of the very few books about the structure of normative thought rather than its content and thereby considerably enriched this universe of discourse. For that alone even those who profoundly disagree with him should applaud.—DON R. BOWEN, *Case Western Reserve University.*

The Political Theory of Local Government. BY W. HARDY WICHWAR. (Columbia: University of South Carolina Press, 1970. Pp. 118. $5.95.)

"Is Local Government self-government, or is it a creature of the state?" Professor Wickwar's little book is a timely contribution to this long debated question which is still argued in legislative, constituent, and judicial bodies. In tracing the evolution of the concept of local sovereignty from Western Europe's middle ages to the present, he sees local government as "an essential link between the central government and the people, and one of the characteristics therefore of all the states that have made the modern world." Moreover, he points out, local government has evolved with the evolution of government as a whole, having a phase in its evolution corresponding to each phase in the evolution of the modern state. He distinguishes the characteristic way of thinking of each of these successive phases in order to help clarify the role of local government in the contemporary state.

Since local government is, as the author states, essentially a creature of law, its political theory and legal philosophy have been largely identical. The main concern of the study, then, is the evolving legal philosophy of local government, found to some extent in legislative and judicial decisions but more often in the philosophy of jurists, legal histories, and law reformers. Professor Wickwar concentrates on these ideas as they were expressed in the local institutions which developed.

The medieval concept that local liberty is in practice self-achieved was soon overshadowed by the legal doctrine that local bodies were incorporated by the sovereign. Thus the question of the degree of their remaining liberty was raised. The answer was found and gained general acceptance, from the time of Bodin to Montesquieu, in the theory of "intermediary bodies" which recognized local units as having a certain constitutional status moderating the power of the sovereign while enjoying some delegated sovereign power.

When the conception of the state began to change as a result of the development of utilitarian theory, local government was influenced in several ways. Central reorganization of local government into hierarchical systems of territorial jurisdictions was thought to provide the greatest happiness of the greatest number. This conception of mathematical subdivisions of the state, subject to the constant guidance of competent authority, was applied to the governments of the United Kingdom, India, and France, with emphasis on central responsibility for local government. In America, the application led to the restrictive mid-nineteenth century view known as Dillon's rule, that local government in its many manifestations is created by and subordinate to the state.

The commitment to governmental subdivisions that would cover the entire territory of a state and have upwards responsibility to the authority and competence of the center was soon modified by an ideology of local government that looked backwards to local *self*-government. The influence of various Western European political philosophers, especially German and English, became evident in the romanticization of the local historical unit with elected officials serving gratuitously. The self-governing, usually rural, group was held to have a common law right to protection from central legislative interference. Just as Americans were learning that they were being transformed from a rural into an urban society, they were shocked at the conspicuous failure of their cities and sought municipal home rule, among other reforms, in order to free cities to become strong enough to hold their own in the general interest against strong private interests.

Today in the technologically developed countries of the North Atlantic, major attention has been given less to the freedom left by the state to the local community than to the services rendered by government, centrally promoted but locally executed. A concept of neighborhood arising in the face of the exploding megalopolis may force the central government to make a redetermination of the territorial subdivisions within which people can best take responsibility for meeting their common needs. At the same time, stress on the efficiency of local government is being replaced by emphasis on the techniques of relationships between central and local government, with a resulting need for coordination at the center.

In the final chapter of this study, the author describes local government as an "article of export," one of the "many ideas and institutions that were to give this world a newer approach to cultural unity than it had ever known before." The variety of forms of the article exported from its homelands has varied ever more widely as each has become a part of a very different political system.

Professor Wickwar's unpretentious study, while not answering as such the initial question raised, leaves no doubt that at the present point of its evolution, the central-local relationship is marking out a new path, one which seems appropriate and desirable in technologically developed nations and which may enable us to understand more clearly the institutional problems facing the developing world.

Beginning with sixteenth century writers, the author draws on many of the important contributors to the theory of government and of local government in particular. Whether or not he has extracted the significant aspects of their thought concerning local government is a decision which might be questioned by a political theorist. Nonetheless, for the student of local government his survey of the broad sweep of political thought concerning the question of local sovereignty provides a view of the entire forest which might otherwise be lost among the trees.

Professor Wickwar is one of the very few scholars who has given attention to the political theory of local government. The small number of others making similar studies, such as Anwar Syed in his *Political Theory of American Local Government*, have made presentations much more limited in scope and substance. This book is not only a valuable contribution to local government theory but is also apparently unique in its conception.—ALICE L. EBEL, *Illinois State University*.

Totalitarianism in Perspective: Three Views. BY CARL J. FRIEDRICH, MICHAEL CURTIS, BENJAMIN R. BARBER. (New York: Praeger Publishers, 1969. Pp. 164. $6.00.)

The concept "totalitarianism," as used by serious students of authoritarian regimes, was developed primarily in response to a sense that Soviet Russia (at least under Stalin) and Nazi Germany were essentially similar political systems and at the same time qualitatively different from traditional dictatorships. By whatever criterion or criteria that scholars in the 1940s and 1950s chose to employ in order to differentiate among political systems, the two dictatorships seemed fundamentally unique—or at least to combine a series of traits manifest in other polities in an unprecedented fashion. Teleologically, the regimes seemed to entertain and seriously aspire to uniquely all-encompassing goals. There was enough substance in the rhetoric of the building of a new Soviet man, for instance, as to suggest that these were novelly purposive political systems. Instrumentally the regimes seemed to have developed a new form of rule in that terror was used systematically as a political instrument for engendering the rapid circulation of elites and the atomization of the masses; terror was, in Merle Fainsod's work, the lynchpin of the totalitarian system. Behaviorally, also, Nazi Germany and Stalinist Russia were thought to be set off from other authoritarian regimes. In Hitler's Germany and Stalin's Russia, the traditional distinction between state and society, between politicization and socialization, was erased. No longer was mere compliance tolerable; for the first time affirmation became the norm and affect totally mobilized.

In the 1960s, however, scholars began seriously to question the usefulness of totalitarianism as an analytical construct. In a professional climate in which developmental constructs were gaining the ascendancy, the static, ahistorical quality of most (though by no means all) theorizing about totalitarianism became more readily apparent. The conceptual fuzziness which permeates most efforts to define totalitarianism, similarly, was thought less tolerable as professionals became more preoccupied with the tasks of operationalism. More importantly perhaps, events in the post-Stalinist Soviet Union suggested to virtually every analyst that there were severe limits to traditional totalitarian analysis. At a minimum it was suggested that reconceptualization was in order. Efforts were made as a result to relate patterns of totalitarianism either to levels of economic or political development or to the duration of a regime in power. Many, however, went beyond the advocacy of reconceptualization. For these specialists the issue has become whether there is heuristic utility in classifying most contemporary east European communist states, or, for that matter, the present-day Soviet Union as totalitarian. Finally, some have argued that totalitarianism is of dubious usefulness even in the study of Stalin's Russia and Hitler's Germany and occasionally assert that the concept was propagated in the United States in order to provide the ideological rationale for the cold war.

The three essays contained in the slim volume under review are in different ways evidence of the unease which scholars now feel about "totalitarianism." The essay by Carl Friedrich, for instance, constitutes something of a departure from his earlier positions. Friedrich is now of the view that the "regimes of Stalin and Hitler, far from providing the typical model of a totalitarian dictatorship were rather extreme aberrations." He has seen fit, moreover, to modify his definition of the totalitarian syndrome in order to reflect "some of the evolving theory and practice of totalitarian dictatorship." Professor Curtis takes a somewhat different tack. He may be included among those who argue that there is such a phenomenon as totalitarianism, that there have been totalitarian regimes in the past, and that this concept is of utility in studying the Peoples' Republic of China. He does not, however, consider the contemporary Soviet Union totalitarian. Soviet reality in the 1960s, he asserts, has deviated sufficiently from the totalitarian model stipulated by Friedrich fifteen years ago so as to render improper a decision to include the Soviet Union within the totalitarian rubric. Mr. Barber, finally, would excise the word from the vocabulary of serious political analysis. He appears attracted by the notion that totalitarianism is primarily a pejorative rather than an analytical term; his somewhat discursive essay provides ample evidence that definitions of totalitarianism are numerous, contradictory, and polemical.

Regrettably, however, the book is as significant as an illustration of why "totalitarianism" has become increasingly suspect as a term of art than as evidence of professional unease about the term. Friedrich's essay is particularly vulnerable on this score. For instance, notwithstanding all that we know about the existence of ongoing conflict within the Politburo, among key sub-elites, among formal institutions, as well as between the regime and such major mass elements as the non-Russian nationalities, Freidrich's basic image of Soviet politics in the 1960's is that of consensus—an assessment which is bound to lead readers to inquire whether the totalitarian model facilitates the acquisition of data about the nature of Soviet politics.

He continues to maintain that totalitarianism is a unique form of rule. In practice, however, it turns out that the term is largely a pejorative one used to designate a bevy of authoritarian regimes. It is not quite clear whether Friedrich considers contemporary Yugoslavia totalitarian or not; his statements in this regard are self-contradictory. One is, however, left to wonder what the criteria for inclusion and exclusion are which separate Nkruma's Ghana, Sukarno's Indonesia, Gomulka's Poland, Nazi Germany, and the Soviet Union—all designated totalitarian by Friedrich—from other classes of authoritarian regimes. Furthermore, it is difficult to know how a communist regime could cease to be totalitarian in Friedrich's scheme of things unless it became basically pluralist—which leads one to think that in the case of communist polities, "authoritarian" and "totalitarian" are for Friedrich essentially indistinguishable. Indeed one sometimes gains the impression that Friedrich's effort to adjust theory to practice is little more than an effort to make sure that the Soviet Union fits the definition totalitarian, no matter what changes occur. One would have thought the normal procedure would be to take a definition as given and then reach a considered judgment as to whether a particular state at a particular time sufficiently approximated the archetype as to be retained within a particular rubric. (In this respect Curtis' essay is far more in keeping with the canons of scientific inquiry).

Regrettably, too, the Barker and Curtis essays—while not subject to the reservations expressed about the Friedrich piece—contain little that professionals will find novel, a fact for which Barber and Curtis should not be greatly faulted. Curtis' field is largely Western Europe, while Barber's main interest evidentally is in political philosophy. As a result, the volume will be primarily useful for beginning students of comparative politics. Because there are so many controversial aspects to the book, its major attraction for the faculty member will likely be as a corollary text to assign in order to have something to attack in one's lectures.—WILLIAM ZIMMERMAN, *The University of Michigan.*

Ideorogi-jidai no Ōkon [The End of Ideology], JERZY J. WIATR, translated into Japanese by HIROSHI BANDO. (Tokyo: Godoshuppansa 1968. Pp. 292. Y800.)

This book, written in Polish, was translated into Japanese by Professor Hiroshi Bando who studied at the University of Warsaw for three years. The original title of the book is *Czy Zmierzch ery ideologii?* The book was first published in Warsaw in 1966. When a book written in Polish and translated into Japanese is reviewed in English for an American audience, it is very possible that some misunderstanding may have occurred somewhere along the line.

The book is divided into five chapters: "The End of Ideology and Sociology," "The Concept of Ideology," "The End of Ideology in the West: A Myth or a Reality?" "The Problem of 'De-ideologization' in Poland," and "The Meaning of Ideology in Social Life."

Professor Wiatr critically analyzes the end-of-ideology hypothesis as presented by such Western social scientists as Raymond Aaron, Daniel Bell, and Seymour M. Lipset. His examination of the hypothesis does not stop at the conceptual level; he cites empirical works of Himlstrand, Torgersen, and others to strengthen his position as critic. Aaron is criticized rather severely from the point of view of Wiatr's own Marxian sociology. Having written his doctoral dissertation on the sociology of American racial problems, he demonstrates his knowledge of the works of Western social scientists throughout the book, particularly in Chapters 2 and 3.

Advocates of the end-of-ideology hypothesis contend that the functional importance of ideology depends upon the extent of economic development. In other words, economic development brings about the end of ideology in all countries. Wiatr would assume, rather, that general social conditions and the degree of social stability in particular form an important independent variable affecting the importance of ideology in a given country. The present disturbance in the United

States as symbolized by the rise of many radical organizations, such as the Black Panthers, seems to suggest that Wiatr's approach has had more predictive validity than that of Aaron and others. Ideology is far from dead in the United States today. As I write this review, two buildings of the University of Hawaii are being peacefully occupied by students and faculty members who are opposed to the present University policy of giving academic credits for R.O.T.C. training. My conversations with the protesters lead me to conclude that they are a highly ideologically oriented group, who believe they are doing their part toward the realization of what they consider to be a more desirable society in the United States. I might also add that there are people gathered at the State Capital who are protesting the protesters. There seems to be empirical ground for Wiatr's position in that there is an association between the degree of social unrest or conflict and the importance of ideology. Economic development in the United States in the late 1960's certainly did not bring about a decline of ideology in American political culture. On the contrary, despite the existence of the "silent majority," there is a trend toward a rise in ideology particularly among Black Americans and youth. Our recent concern over ecological issues presents an analytical problem in the sense that it does not constitute any part of ideology as understood in conventional terms. It could be that this is an example of what Yrjo Littunen refers to as the beginning of a new ideology.

The section on Poland (Chapter 4) presented the author with a problem owing to the lack of relevant empirical work. I believe that he could have presented much more forceful evidence to refute the hypothesis, had he been able to base this chapter on his own survey of Polish citizens. In fact, he could have made a more original contribution to the literature if he himself had done some survey work in Poland prior to writing this book.

In the final chapter, he argues from the Marxian viewpoint that an end to ideology is detrimental to the development of a society and serves to preserve the status quo. He goes further to suggest, under the subheading of "Ideology and Science," that ideological inspiration plays a crucial role in the development of science. He ends the book by emphasizing the importance of ideology in the development of a society. Ideology, by affecting our feeling and rational thinking, will lead us down the road toward progress. Thus, he not only rejects the end-of-ideology hypothesis which LaPalombara and others have attempted to formulate, but he emphasizes the functional use of ideology not only in developing a society but also in advancing our scientific knowledge.

Professor LaPalombara (March, 1966, this Review) and Professor Wiatr seem to be in agreement that the end-of-ideology hypothesis is not applicable to Italy. The former confines himself to the Italian situation, whereas the latter finds it inapplicable elsewhere. However, Wiatr admits that there seems to be some truth to the end-of-ideology hypothesis in the United States and some other Western nations. He, in this sense, certainly cannot be accused of being dogmatic.

Although I sympathize very much with the translator in his difficult task of translating this book into Japanese, including as it does quotations which originally appeared in English, French, Russian, and other languages, I must report that I found a few errors. For example, the book refers to the University of Michigan as "Ann Arbor University" (pp. 109 and 110). Since Professor Wiatr spent some time at the University of Michigan, it is difficult to imagine that he made the error. In any case, these errors are not crucial.

The book is not a polemic work of the sort which is found in abundance in Marxian literature. Rather, it presents a scholarly effort to challenge the end-of-ideology hypothesis and to analyze the role of ideology in societies.—YASUMASA KURODA, *University of Hawaii.*

The Politics of History. BY HOWARD ZINN. (Boston: Beacon Press, 1970. Pp. 390. $7.50.)

The purpose of Howard Zinn's excellent collection of essays on history and historiography is to draw attention by both analysis and precept to "the consequences *in action* of historical writing. The meaning . . . of a writer will be found not just in what he intends to say, or what he does literally say, but in the effect of his writing on living beings." (p. 279) "The Politics of History," then, is a literal title, referring not to political events in time past but to the current activities as historians (and social scientists) themselves, Zinn's own included. For in a world, as he puts it, "where children are still not safe from starvation or bombs," it is willy-nilly a political act to engage one's sympathies with or withhold them from the actors who have made the present and are making the future, and who are the objects of our study; indeed, the very choice by academics of a subject matter itself is also a political act. Thus Zinn rejects the canon of so-called objectivity in research, for since all social studies whose interpretation bears on our assessment of our own condition are *ipso facto* political, "objectivity" is only a mask with which we hide the real social consequences of what we are doing and saying.

In the beginning and concluding theoretical essays that form the framework of this collection, Zinn criticizes *inter alia:* 1) history-writing as *supposedly* value-free narrative about events whose interpretation is significant for us as citizens today (and which therefore can't possibly be genuinely "value-free"); 2) history-writing about such events, in the analysis of which the historian shows the *wrong* human sympathies, rather than pretending to have none; 3) the conventional wisdom particularly in the field of American History, which fobs apologetics off as "historical knowledge" to be passed on from generation to generation; 4) pedantic historiographical controversy about the supposedly best way to insure the discovery of those chimeras "science" and "objectivity" in historical studies, a controversy which otherwise intelligent academics engage in as a substitute for committing themselves to the discussion of important substantive issues; and 5) history-writing as value-free narrative about events of mere antiquarian interest. (Zinn, I think, overdoes this last point. The tellers of socially trivial tales are the true members of a very narrow, specialized calling related to Zinn's in name only; no more than shoemakers ought they to be scolded for not being critical intellectuals—no more than Agatha Christie ought to be scolded for not being James Joyce.)

In all these cases the clear point Zinn makes is that historians and social scientists are political men (one of Zinn's essays is entitled "Knowledge as a Form of Power"), functioning as apologists for the American status quo. Some carry out this function tacitly—through professional narrow-mindedness (4 and 5), methodological naivete (1 and 4), or lack of commitment to piercing beneath the veil of appearances (3); others carry it out overtly (2) by opposing the drastic social changes Zinn feels are necessary (and will be felt as necessary, he suggests, by anyone who detaches himself from the interests of the wealthy and powerful of the world). Unfortunately there is some confusion in Zinn's theoretical discussion of these points: the concluding essays are pitched almost entirely at the level of methodology, when in fact the more important criticism is that so many academics lack independence of intellect or a feeling for justice (3 and 2)—and these are not methodological problems at all.

This confusion in an otherwise brilliant discussion is more than compensated for by the uniform excellence of the substantive essays that form the bulk of this book. It is in these essays that Zinn both particularizes his critique of status quo apologetics and—what is more rarely done—offers his own, competing version of history in the service of social change rather than history in the service of the current world and national division of powers.

Roughly speaking, these essays fall into two groups. First those collected under the heading "Nationalism" vigorously attack the credentials of American liberalism in foreign policy. In a well-known article on the Vietnam War and with especial forcefulness in a chapter of historical summary entitled "Aggressive Liberalism," Zinn undercuts the pieties with which Americans assure themselves that they have been peaceful and reformist in external affairs (including especially the affairs of this continent), rather than warlike and reactionary. Of course this argument is familiar by now; but it is Zinn among others who helped to make it so familiar.

The second group of essays forms a long and often exciting example of what Jesse Lemisch has called "History from the Bottom Up." This kind of history, unlike most of our standard histories, is not based on the perceptions of political and social leaders, intellectuals and other beneficiaries of the American success story; rather, it is built around an attempt to understand the lives and hopes of the American underclass, and of our relatively few but enduring rebels; the "success" of American liberalism on the domestic scene is not presumed (as it is by "consensus" historians) but is precisely the fundamental point in question. In essays on inequality in American history, the 1914 massacre of striking mineworkers at Ludlow, Colorado, Fiorello LaGuardia, the New Deal, the Abolitionists, racism in America, and the suppression of mass civil rights demonstrations in Albany, Georgia, Zinn returns to this point over and over again. There has always been a multitude of the excluded and oppressed in America, and their oppression has often been (and still is) terrible; liberal meliorism has often been of little help to this class and on many occasions, especially when race has been involved, has not even been benign (Zinn demonstrates conclusively that the Kennedy Administration, by its unwillingness to protect peaceful protestors against savage attack, combined with its willingness to prosecute *them* when they defended themselves, allowed and even fostered a "pattern of brutality against the Negro" in the Deep South—and today everywhere). Finally, he reveals American history-writing at its most complacent and methodologically inept in its treatment of the American radical tradition, as he shows that our historians have followed the pattern of psychoanalyzing the *latent motives* of radicals, but unquestioningly accepting the *manifest statements of principle* of the ruling class—and of the historians themselves! Writing about Lewis Feuer's analysis of the Berkeley rebels Zinn notes that "Biopsychological 'causes' of human catastrophe are . . . marvelous reinforcers of the going order because they have no operable corrective . . . Explanations of social events by castration complexes insure impotency" (p. 165).

In sum, Zinn writes from the same standpoint that Christian Bay justifies in his *The Structure of Freedom*: a society—and for an historian like

Zinn the past of a society—is to be judged not by the power and freedom available to its best-off citizens, or even to the average citizen, but by the kind of life available to its *outsiders*. Zinn, like several others now doing the same kind of work in diverse areas of research—Lemisch, Stephen Thernstrom, Michael Parenti, Peter Bachrach, Louis Lipsitz, and Robert Coles for example—has made a major contribution to a more balanced judgment of this society. It may be time for the practitioners of this kind of scholarship to come together and create their own standard texts to spread their unconventional wisdom.—PHILIP GREEN, *Smith College*.

Interest-Groups. BY GRAHAM WOOTTON. (Englewood Cliffs: Prentice-Hall, Inc., 1969. Pp. 116. $1.95 paper, $4.95 cloth.)

This textbook is introductory, comparative, theoretical and short. Descriptions of group behavior appear only as examples, the purpose being to provide a framework within which students may analyse and compare interest groups, their activities and impact. The guiding formulation is:

$$PA \rightarrow T : G$$

which means that Private Actors (other than political parties) seek to influence governmental Targets to secure certain Goals.

Wootton starts with an incursion into semantics, examining several definitions of "interest group" and purposes served by defining it. He concludes that these have led to a "muddle." He then proposes the device of a "real" (rather than "literary") definition, one which flows from Observers' efforts to explain actions in their context and to relate them to other actions and situations in order to understand and interpret them; e.g., "handsome is as handsome does." Then he suggests that the symbol PA may stand for whatever agency one feels it necessary to study.

Thereupon he introduces Parsonian pattern variables, somewhat adapted, to represent the social context in which group actions take place. There are Economic (adaptive) groups, e.g., factories, farms and mines; Integrative groups, e.g., legal institutions; and Cultural (pattern maintenance) groups, e.g., families, churches and schools. The fourth category, Political, of course, provides the Targets. Other dimensions include latent-manifest, industrial-agrarian, and first, second and third order groups, from operating units up to peak associations. These typologies lead into his "general hypothesis" (p. 38) that "line of importance of interest groups *tends* to follow the contours of manifest functional significance."

He further makes clear the difficulty of classifying some groups by this set of variables, note that Targets, such as legislatures and executives, may not perceive the latent functional significance of groups in the same way as Observer does, and hypothesizes that ascribed status-groups such as veterans appear to be influential beyond their functional contribution. He discusses the "anchorage" of certain front organizations in more basic interests. Then he sets forth the variables to be analyzed: The Actor group and its Goals and claims, the Targets and their constitutional and electoral base, the Political Parties, the impinging activities of other groups, and the Political Culture in which it all takes place. Adapting Dahl, he evaluates influence in terms of the number of Targets a set of Private Actors can affect, how far it can move them, at what cost to T in terms of altered normative commitments, and the scope of action, i.e., the variety of policies that can be influenced. He classifies the endowments of groups as money, organization, control over jobs, expert knowledge, votes and prestige (note the absence of ideological appeal), and observes that the willingness to devote these endowments to political action as well as the ability to employ them efficiently are critical variables. He examines the responsiveness of Targets, and their occasional willingness to incite group pressures upon themselves and other Targets. Thus he covers many of the concepts of traditional and contemporary interest group studies, incorporating them into an integrated scheme of analysis.

This is an introductory text, and should be evaluated as such. First, its assets. Wootton regularly, explicitly reminds the student that he is Observer, that data do not speak for themselves, that phenomena as perceived by Actors or Targets may be evaluated differently. He distinguishes what is to be established empirically from what is assumed or defined. He relates his scheme to those of other social and political theorists, which is useful to undergraduates, who usually are not adept at transferring what they have learned in one course or discipline over to another. The important variables are listed, explained and appraised. The examples he gives are set in a comparative framework—usually British vs. American—and they serve to keep theory and data in touch with one another.

The style—brisk, straightforward, almost conversational—should be a joy to students. Why can't American scholars write like this? An example, apropos of functional requisites (p. 32): "As for survival, we can judge that for an organism, but what is it—and what is death—for a society or social system? In the functionalist canon the sweet simplicity of death is lacking."

Among its shortcomings must be listed the evasive character of the hypotheses. The central one (see above) is essentially a metaphor in which "lines" tend to follow "contours." I am unable to see how this or any other metaphorical proposition can be falsified. His subsidiary hypothesis, that business groups are powerful because production is functionally important, has a tinge of circularity about it. But perhaps it is unfair to expect testable hypotheses from functional analysis.

Though quite suitable in style and content as an introductory text for theoretical analysis, it probably will work better with students who have acquired some sophistication. It would be better to start a budding social scientist out along the well-mapped terrain of comparative political parties than to throw him into the labyrinth of group theory where some senior scholars have perished.

My final cavil is not with this book but with interest group studies in general. They derive from and remain too largely preoccupied with business interests and the counterforce of organized labor. They have not come to grips with recent events in the United States. Following the theory presented here, especially the portion dealing with endowments of groups, one could reasonably have predicted that the Economic Opportunity Act would not pass Congress, that the Welfare Rights Organization would be impotent and that college students would exert a negligible impact upon foreign and defense policy.

On balance, this looks like a good textbook.—William Buchanan, *Washington and Lee University.*

The Anti-Communist Impulse. By Michael Parenti. (New York: Random House. 1970. Pp. 333. $7.95.)

This book by a young political scientist of a radical bent is directed not to specialists but to a general readership. Although it is something more than a tract (perhaps also something less) its evident purpose (a laudable one, as far as it goes, in the opinion of this reviewer) is to influence contemporary politics, to persuade people, to change the minds of readers about current affairs. "My Hope," Mr. Parenti writes, "is that this book will help others entertain new images and new realities of the kind that might bring us to a saner, less tragic political world."

The means Mr. Parenti has used to try to fulfill this worthy hope is a kind of hortatory and thoroughly disapproving analysis of what he sees to be a distinct social-political phenomenon, "anti-communism" (that is, *American* anti-communism; he does not examine international varieties, to deal with which might complicate his argument considerably). This "anti-communism," he says sweepingly, "is the most powerful political force in the world . . . it commands the psychic and material resources of the most potent industrial-military arsenal in the history of mankind. Its forces are deployed on every continent, its influence is felt in every major region, and it is capable of acts which—when ascribed to the communists—are considered violent and venal. Our fear that communism might someday take over most of the world blinds us to the fact that anti-communism already has. If America has an ideology, or a national purpose, it is anti-communism. Hundreds of billions of dollars have been expended, and hundreds of thousands of lives have been sacrificed on its behalf." If the reader will look closely at the use of the word "it" and "its" in those sentences—"it" commands, "Its" forces are deployed, "it" is capable of violent, venal acts —he will see what Mr. Parenti's central idea and rhetorical strategy is, and perhaps also (depending maybe in some part on his own political opinions) what is wrong with it.

To this reviewer Mr. Parenti in parts of this book seems to be a more sophisticated Fred Schwarz, stood on his head. (Fred Schwarz, the reader may remember, is that "You can trust the Communists—to be Communists" man who in the early sixties went around the country lecturing at "anti-communist" schools, explaining that one must "teach" about the omnipresent, terrible, almost omnipotent thing "communism" in the same way that medical schools teach about cancer). It may have been a deliberate ploy of Mr. Parenti's, to do with "anticommunism" just what, as he says, the Americans he is criticizing have done with "communism." But the result of adopting this device, it seems to this reviewer, is to give to his own book something of the polemical distortion, the over concentration on ideology divorced from events. The blurring together of quite distinct positions, the unhistorical scoring of points by people living in one situation agaist people living in a quite differet one, that characterize the phenomenon he attacks.

This is one of these controversial opinion-reflecting books of which it will be hard to find an objective review. In the present intellectual climate no doubt it will receive some favorable ones. In an attempt to be fair the present reviewer should state (what is already evident) that he is less than sympathetic to the political position from which the book is written; the reader should make whatever adjustments he thinks that fact requires. But surely Mr. Parenti intends not only to reinforce those who already agree with him but also to persuade and educate a reader with somewhat different opinions, and one may therefore usefully state why that did not work in the present case. Obviously the first question is, how does the writer relate this obsessive self-deluding American "anti-Communism" that he describes to objective realities in world politics? The Soviet Union, and Communist China, and communist movements elsewhere, after all, do exist, and do have a real history, however distorted popular American perceptions may be. What is the *corrected* perception of the events and realities of cold war history? Mr. Parenti's book has an unsatisfactorily tangential relation to that question, which gets short shrift. He focuses instead rather too exclusively on America's national self-righteousness, and moral imperialism, and self-justifying myths, and so on.

The second question is, how does the book relate *popular* American anti-communism (of which indeed a separated opinion-study could be made) to American policy making and to serious American interpreters of world politics? The answer is, that he binds them all in one bundle of simplified obsessive anti-communism. The cover of the book carries ugly mask-pictures not only of Senators Joseph McCarthy and Karl Mundt, but also of Secretaries Dulles and Forrestal, and also Presidents Eisenhower and Truman. There will be readers who will find it meaningful to regard Joseph McCarthy and Dean Acheson as part of the same thought world, but this reviewer is not one of them.

A point that Mr. Parenti very much wants to make—he argues some of these rather well—is that "liberals" during the cold war period reinforced the nation's vulgar and obsessive anti-communism by regularly acceding to it, reiterating how anti-communist they are too. There is truth in that. But Mr. Parenti does not assess accurately how much of that liberal anti-communism was genuine, based in brutal experience and not merely on public relations considerations. And a reader who remembers the time cannot in his heart bring himself to join Mr. Parenti in condemning—to choose just one striking example—Owen Lattimore, after his ordeal, for listing in his book all the anti-communist things he had written and said. Such a reader cannot believe that this book will help much in bringing "a saner, less tragic political world."—William Lee Miller, *Indiana University.*

Politics and the Community of Science. By Joseph Haberer. (New York: Van Nostrand Reinhold Company, 1969, Pp. 337, $3.50.)

In the light of steadily increasing concern over questions relating to the social responsibility of scientists, this book has been published at a most appropriate time. For its main purpose is "to examine modern science in the light of some fundamental political and moral problems with which its rise to a major institution of society has confronted us." (p. i)

For Professor Haberer, science is not just one of society's major institutions. In his opinion it is quite possible that science is becoming the "leading institution" of society. In large part his judgment is based on the view that "modern man looks not to the Church or the State, nor even to the Corporation or the Military, but to Science and the scientist for answers to his besetting problems." (p. 3) There is good reason to share the latter view. Ironically, the effect of the case that Haberer argues throughout his book is to show that it is the nation-state and not science, the emerging "leading institution," which is the object of the ultimate loyalties of scientists themselves. As the author himself points out, scientists have generally been much more prepared to make serious sacrifices for the sake of their nation-states than for the sake of science or the scientific community. Thus, to say that science is becoming the leading institution of society is not to say that it constitutes a community which claims the ultimate loyalties of its members.

The author begins his examination of modern science by describing what he considers as the two main conceptions or models of science as a community and the role of that community within the political order. These two models are those advanced by, and embodied in, the behavior of Bacon and Descartes.

For the author the significance of Bacon and Descartes is that they

> laid the philosophical and institutional bases for the whole enterprise of modern science [and] provided the rationale and the models for the community of science, as well as a pattern and vision for its future development. (p. 20)

While the ideas of Bacon and Descartes differ in certain crucial respects, those relating to the role of the scientist vis-à-vis the State are similar. As the author puts it,

> In any serious confrontation with State or Church . . . they [Bacon and Descartes] both advocated a tactic of prudential acquiescence: their theory and their conduct posited *retreat or an apparent acquiescence* as the appropriate response to danger. (p. 80)

Haberer disclaims the possibility of unequivocally establishing any causal relationship between the ideas of Bacon and Descartes and the behavior of scientists in specific situations involving the State. However, his approach is based on an assumption that those ideas and the institutional development of science which was based on them inevitably have some influence even on the behavior of scientists in recent times.

The method by which the existence of a pattern of prudential acquiescence is established consists of an examination of the relationship of Bacon and Descartes to the political authorities of their day and the responses of two scientific communities, the German and the American, to sets of events which constituted major crises for those communities or their leaders.

The author's judgment on the response of the scientific community to crises in a harsh one. He concludes that

> when the interests and values of science confront those of politics, the community of science has ultimately tended to acquiesce with astonishing ease, permitting, with only token opposition, the destruction of colleagues and scientific institutions, and the perpetration of obvious assaults upon the integrity of science itself. (p. 310)

Thus the German scientific community acquiesced in "Nazi attacks on the integrity of science, Jewish colleagues and the traditional autonomy of university life," (p. 155) and American "scientists' public response on behalf of [J. Robert] Oppenheimer can be characterized as weak, nonactive, and tenuous." (p. 293)

For Haberer, the failure of the community of science to defend itself against the State suggests that members of the community may in fact be committed to no "ultimate values," such as the search for truth, and thus have functioned as guns-for-hire in the interest of whatever values the State chooses as desirable. Scientists are urged to face the question of their "social responsibility" and to abandon the outdated assumption that "progress" is an inevitable, and desirable, product of science.

The book succeeds in making explicit and clarifying important questions involving the relationship of science and governments. It also convincingly demonstrates that the scientific community has acquiesced to governments at crucial points in history and has generally been unwilling to face up the question of its "social responsibility." Furthermore, it calls attention, explicitly or implicitly, to a number of areas which might profitably be the subject of future research by political scientists employing a wide variety of methods. One of the more significant of these areas is that involving the socialization of scientists and the implications of that socialization for their attitudes and behavior vis-à-vis government and public affairs.

In terms of the book's objectives, its failures are relatively minor and in large part unavoidable consequences of the breadth of its scope and the nature of its methods. For example, Professor Haberer himself notes that previous literature on the relationships between science and government suffers from an assumption of the universality of conclusions drawn from studies of single countries. In spite of the fact that his work encompasses a time period of more than 300 years and a geographical range of four countries, his evidence derives from a small number of case studies and is subject to some of the same kinds of objections he makes to previous work. Nevertheless, the inclusion of historical and comparative perspectives represents a significant addition to the literature.—James E. Underwood, *Union College.*

Revolutionists in London: A Study of Five Unorthodox Socialists. By James W. Hulse. (New York: Oxford University Press, 1970. Pp. 246. $6.00.)

Professor Hulse examines the impact of tolerant late Victorian London and the company of each other on five radicals: the Russian nihilist-activist, Sergi Stepniak; the Russian anarchist-philosopher, Peter Kropotkin; the German Marxist, Eduard Bernstein, and the two British men-of-letters, William Morris and George Bernard Shaw. In the receptive London intellectual environment the three radical exiles changed considerably in the direction of democratic liberalism. The two Britishers were, in turn, influenced toward more radical positions by their contact with the expatriates.

Hulse sets out on "an experiment in synthesis" with the five thinkers "to understand each of them better by looking at them together, comparing them as it were with one another rather than describing each of them against a more generalized background" (22). His objective is not to duplicate the works of G. D. H. Cole, with its encyclopedic classification, or of others who have focused on the organizational machinations of radicals. Because he found "people more interesting than petty factional struggles and men of integrity more fascinating than men of power" (vii), Hulse became disappointed with the conventional accounts of Socialism, Communism and Anarchism which focus on institutional and doctrinal disputes and neglect the personalities involved.

Although Hulse puts forth his work as a synthesis, he devotes most of his study to each thinker separately or to their individual influences on each other. In only two places (22–24 and the final paragraph, 228) does he turn his attention to the five together. The men did not think of themselves as a group. They worked alone and contacted each other singly. If they ever came together at one time, it was likely a chance meeting, perhaps at a public debate or in the home of Frederich Engels.

Lines of influence among the five, furthermore, were uneven. The Morris-Shaw relationship was close and well-known; Kropotkin had frequent contact with Morris and was admired by Shaw; and Stepniak seems to have been friendly with each of the others. Bernstein had a warm friendship with Stepniak and his ideas have some parallels with Kropotkin's, but his contacts with Morris and Shaw seem to have been minimal.

Sergei Stepniak-Kravchinskii has been bypassed by most interpreters of revolutionary socialist thought, including Cole. Hulse makes it apparent the Stepniak has been wrongly ignored. Stepniak, a practicing terrorist, had assassinated a Russian adjutant general. He saw this and other terrorist deeds as defensive, collective acts of reformers, the necessary logical results of Tsarist bureaucracy. His appeal to the English intellectual community on behalf of the Russian peasants won him support in Britain and America. At the same time his last dozen years in London mellowed his terrorist zeal. The London atmosphere and the success of German Social Democrats convinced Stepniak that violence was not necessary to advance socialism, even under authoritarian government. Before he died, in his mid-forties, in 1895, he had reached revisionist conclusions similar to those of his friend, Bernstein. Since little has been written in English about Stepniak, Hulse's work provides an introduction to this apparently influential nihilist. Hulse uncovers enough to arouse curiosity but not enough to provide a full understanding of Stepniak, opening the door for further work.

Kropotkin and Bernstein also underwent transitions from militance to moderation during their London years. Kropotkin was in London thirty-one years before returning to Russia during the 1917 revolution. Bernstein stayed in London thirteen years, from 1888 until he again became involved in politics in Germany. Hulse shows that Kropotkin's revolutionary anarchism, although resolute throughout his life, ended in frustration in Bolshevist Russia. Nevertheless he did not dilute his anti-capitalist, anti-statist ideas and maintained that a revolutionist had an ethical obligation to restrain hate and irrational violence.

Bernstein was not known as a revisionist until toward the end of his stay in London, but Hulse maintains Bernstein's revisionism did not result from an intellectual debt to the Fabians. Instead, it was more the general English atmosphere, Bernstein's contact with Engels, his work on Ferdinand Lassalle's writings, and his interest in seventeenth-century English revolutionary history that made him sceptical of final dogmas. 'Socialism got a good history from Bernstein's studies of the English revolutions, but it also got a man who had developed a taste for heresy" (143).

The chapters covering Morris and Shaw do not seem as well developed as the others. Morris and Shaw might have been compared to the three exiles without separate treatment. Neither underwent the dramatic transformations of Stepniak and Bernstein or the gradual mellowing of Kropotkin. Hulse sees Morris as "an English equivalent of the *narodniki*" (80) but never rigid in his position, receptive to the ideas of the Marxists as well as of Stepniak, Kropotkin, and Shaw. "Shaw's Socialism," Hulse concludes, "was an amalgamation of his early admiration of Marx's denunciation of capitalism, his preference for the moderate procedures of the Fabians, and his anarchist yearning for the total transformation of society" (228).

In general Hulse presents each of the thinkers separately, but he can be credited with making an effort to integrate the ideas, personalities, and intellectual environment of these unorthodox radicals in Victorian London.—Ronald Christenson, *Gustavus Adolphus College.*

Montesquieu's System of Natural Government. By Henry J. Merry. (West Lafayette, Indiana: Purdue University Studies, 1970. Pp. 414. $8.50.)

We cannot but be amazed by the breadth of Montesquieu's scholarship. He seems to present a comprehensive view of politics by comprehending an almost bewildering variety of societies and practices, and by enabling the reader to supply variations left out. If Montesquieu teaches by example, we are faced with the challenge that one must become a truly cosmopolitan scholar to understand either Montesquieu or politics.

Professor Merry seeks to correct the parochialism of American interpretations of Montesquieu's works. Heeding the warning in the introduction to *The Spirit of the Laws* that "a great many truths will not appear till we have seen the chain which connects them with others," he searches for the unity that will enable one to understand the diversity of subjects contained in Montesquieu's great work.

Our parochialism is excessive legalism, the allegedly mortal disease of old-fashioned American political science. This view finds the heart of Montesquieu to be a doctrine of separation of governmental powers as the guarantee of liberty. It vulgarizes Montesquieu and mistakenly lends the authority of a wise man to American institutions whose true origins are mundane. Montesquieu must also be defended against sociologists who see his discussion of climatic influence as but a primitive forerunner of modern sociology. In truth, the ancestor equals or surpasses the progeny. Far from dwelling amid provincial legalism or simplistic climatic determinism, Montesquieu finds his true home in contemporary sociological and psychological sophistication. Merry banishes parochialism by showing that the 18th century baron is at least as sociological as we are.

The coherence of *The Spirit of the Laws* is found to be that of a system: a complex series of interrelationships that have a pattern. The unity is a system because it is composed of relationships between parts, not between parts and the whole. One need not look at the form or end of government. Nor is the pattern of relationships to be found between governmental institutions. Rather, one must view how government stands in relation to natural—that is, sociological and psychological, as opposed to political—factors. While Montesquieu prescribes healthful medicine for France, his deeper intention and accomplishment is to set forth a system of natural government that will give the reasons for every government without judging the ends of any.

The opening chapters of Merry's book summarize Montesquieu's ideas about the nature of man and society. Montesquieu, who was at home among the world's strangest customs, is assumed to share the opinions of man expressed by those two amazed Persian tourists, the tormented Usbek and his companion Rica. The problem discussed can here only be indicated by Montesquieu's remark, "It is well that there has been in the world some good and some bad; without that, one would be driven to leave this life."

The bulk of the book examines *The Spirit of the Laws*. Books XIV–XXXI are discussed first in order to proceed from nonpolitical to political. Since Merry contends that Montesquieu believes the latter does and must reflect the former, he treats the subpolitical first in order that politics may be better understood. He argues that Montesquieu seeks to establish the independence of economics, religion, and civil law from politics and political law in order to unlease the activity stimulated by greed and checked by fear of God, and to replace natural by civil law as the standard of political law.

Read in the light of Montesquieu's treatment of nonpolitical things, books II–X, which have traditionally been regarded as a discussion of forms of government, are interpreted to be an examination of the capabilities, roles, and relations of the three main social classes. Their purpose is to show the need for and foundation of a mixed government that reflects social forces and does not require austere virtues.

The final three chapters, which discuss the books on liberty (XI–XIII), seek to show that both civil and political liberties are primarily nurtured and secured by the proper distribution of political authority among the three social classes rather than by legal separation of powers. Professor Merry finds that the famous discussion of the English Constitution is not central to *The Spirit of the Laws,* but deals with only one kind of political liberty and that not the most important. Furthermore, Montesquieu's natural government cannot be understood, and is distorted, by looking at its end of liberty. A spirit of moderation is more to be sought by mixed government than even liberty.

Professor Merry's book is best when it is closest to Montesquieu. He reminds us of the breadth and subtlety of Montesquieu, and gives us the fruits of reflective hours spent in the baron's company in many provocative interpretations. He has an easy time in rescuing Montesquieu from legalistic Americans, though one might wonder whether there was not some truth in the old prejudice. For if the ends of government are given, is there a more important question than how these ends are achieved? It is true that Montesquieu writes about the spirit, but it is the spirit of the laws.

Merry would have Montesquieu read in our times by showing that he speaks like our times. The use of social science terminology confuses the interpretation and makes Merry say less than he knows. For example, to say that moderation is higher than liberty in Montesquieu's value hierarchy is to neglect the fact that neither moderation nor liberty is a value for Montesquieu, but one a spirit and the other an end of the laws. This confusion prevents Merry from clearly raising and adequately answering the question whose general form might be, "Why does Montesquieu write about the spirit of the laws rather than the end of law?" To say that Montesquieu values the spirit more than the end is no answer.

One is tempted to suggest that Montesquieu should be read in the light of what preceded him rather than in that which followed. The character of the mixed government admired by Montesquieu is never clarified by Merry as it could have been had he compared it to the mixed regime of Aristotle. The opening discussion of the nature of man would have been less a mere collection of opinions had he sharpened the issues by, say, reflecting on the previously quoted remark in the light cast by the *Politics*. Furthermore, the warrant for such comparisons would be Montesquieu's own, as it is he who uses the traditional terminology to change it.

Finally Merry's procedure hides from us the most important lessons of Montesquieu. Montesquieu proceeds from the old classification of governments to the new. In discussing Montesquieu, Merry proceeds from the new sociology and finds the old classification irrelevant. Accepting Merry's interpretation of the character of the new political science, one might wonder whether Montesquieu's order does not indicate that political philosophy leads to sociology, where Merry's order seeks but to reveal Montesquieu as a political sociologist. In portraying Montesquieu as a sociologist, Merry nearly forgets that he was a philosopher. By neglecting the possibility that Montesquieu does not merely present a system, but that he gives the reasons for doing what we now do out of habit, Merry downplays precisely that element of Montesquieu from which our times might learn the most.—GLEN THUROW, *State University of New York at Buffalo.*

Robert's Rules of Order. (New Revised). BY HENRY M. ROBERT, SARAH CORBIN ROBERT, WITH THE ASSISTANCE OF HENRY M. ROBERT III, JAMES W. CLEARY, AND WILLIAM J. EVANS. Glenview, Illinois. Scott Foresman and Co. Pp. 550. 1970.

If it is natural for Americans, at least middle and upper class ones, to join voluntary organizations; it is almost as natural for those organizations to run, at least in principle, in accord with parliamentary procedure. While hundreds of manuals of parliamentary procedure have been published over the years, of which well over a dozen are currently in print; there remains only one *Robert's Rules of Order*. For almost a century, ever since it replaced *Cushing's Manual of Parliamentary Practice* (1844) as the basic parliamentary reference work, to virtually all Americans *Robert's*

Rules IS parliamentary procedure and using any other manual would be sacrilege. Indeed, it has been seriously suggested that only the Bible has had a greater influence on the organizational behavior of Americans. Almost three million copies of *Robert's* various editions have been sold since 1876, which has made it one of the all-time nonfiction best-sellers.

Although General Henry M. Robert has been dead for nearly half-a-century, *Robert's Rules of Order* lives on; and within its pages the ideas of the long-dead general are perpetuated. The present edition, edited by the General's daughter-in-law with the assistance of several others including the General's grandson, is the first complete revision since 1915 and the first new edition in nearly twenty years; but there is nothing in it that would be new to General Robert. It supersedes previous editions, but has been written to be "in complete harmony" with them. Nonetheless, in the words of its inside front cover dustjacket, this edition is "more modern, more complete, more comprehensive, better organized, more clearly presented, more efficient, and FAR EASIER TO USE than any earlier edition," a description with which I am in complete accord.

Robert's Newly Revised is several hundred pages thicker than the 75th Anniversary Edition which it replaces. It incorporates material from General Robert's earlier works, *Parliamentary Practice* and *Pariliamentary Law,* so as to provide for the first time answers to various esoteric questions of procedure in one definitive reference work. Earlier editions of *Robert's Rules* had charts which were impossible to use, with stars, asterisks, footnotes, and fine print enough to confuse even an experienced parliamentarian. This new edition has a forty-eight page center section of charts and tables, readily set off by a different color and heavier stock paper, which is an absolute delight to use for the student seeking quick reference as to form, precedence, and applicability of motions. This new edition also has the pleasant distinction of being reasonably well written and exceptionally well organized. Logical arrangement of material has replaced the old paragraph format and the order of presentation has been designed to be in accord with the natural flow of business at meetings. The ambiguous classification of certain motions has been remedied and the basic classification scheme clearly explained. For each motion, a section in outline form clearly and succinctly sets forth the motion's basic operational characteristics and its uses. All in all, the new edition is far superior to any of its predecessors.

Robert's Rules has an importance to the social scientist which goes well beyond its possible immediate relevance to him as a concerned participant in various group decision processes. Parliamentary procedure, especially that distillation of it which has entered into parliamentary law—those common principles of procedure which are held by the courts to be the applicable to every organization as being essential to legal action or for the protection of members in their individual rights—has been important in shaping and refining basic American notions of due process and majority and minority rights as applied to group activity. Indeed, *Robert's Rules* has been cited as authoritative in a number of court cases. More generally, however, *Robert's Rules* may be regarded as an implicit theory of democracy. For many Americans its procedures are synonymous with practical democracy. In fact, the inside jacket cover calls *Robert's* the "book you will want . . . the book you will need, to help get things done *in accord with the American spirit* (emphasis mine)." Contrasting the procedures in *Robert's* with those practiced by Quakers and with the Quaker inspired "participatory democracy" procedures of the New Left reveals quite different priorities assigned by each to values such as speed of deliberation, unanimity, intensity of preference v. majority rule, mass participation, etc.—raising questions which should be of great interest to any student of democratic theory. *Robert's Rules* is also important to the student of public policy concerned with the impact of rules on outcomes, and is, of course, particularly relevant to the student of legislative behavior. Finally, for the mathematically oriented political scientist, *Robert's Rules* offers for study a remarkable and fascinating system of queueing rules.

In an era where faculty meetings, student-faculty meetings, and mass meetings abound, we do not wish to neglect consideration of *Robert's* as a guide to the parliamentarily perplexed. It is possible for someone to learn parliamentary procedure by reading *Robert's Rules of Order* from cover to cover, but we would certainly not recommend it.* Despite the stated intentions of the editors of this edition to combine in it a definitive reference work and a teaching manual, it is only the former which has been achieved; although this edition is far superior to its predecessors in its accessibility to the parliamentarily uninitiated. Despite vast improvements in organization and in prose style, *Robert's Newly Revised* is, like its predecessors, marred by an archaic terminology (e.g., previous question, lay on the table), some unnecessarily complex and confusing rules (e.g., reconsider, committee of the whole v. informal consideration), and some rules which could best be dispensed with (e.g., move to reconsider and enter on the minutes, object to consideration) which fealty to the dead General Robert and a desire to maintain terminological accord with the U.S. House of Representatives have unfortunately frozen into place. While this new edition is a vast improvement in so many ways, it is also a great disappointment to those who had looked for a genuine revision and modernization of American parliamentary practice. For example, the discussion of procedures which could be adopted in either very small or very large gatherings when ordinary parliamentary procedure becomes unduly cumbersome (either because it is too formal or because it becomes bogged down in the weight of large numbers of participants) is woefully inadequate, despite a considerable improvement in the section on mass meetings; and in general we found some lack of sensitivity to the need to vary procedures in accord with the size and nature of the group. Also, the section on voting procedures reveals no knowledge of recent work on committee election procedures, such as that of Duncan Black, which is highly relevant to a consideration of selection of an appropriate and fair voting procedure. Regardless, however, of its shortcomings, *Robert's Rules of Order Newly Revised* is definitely a book with which most political scientists should become acquainted.—Bernard N. Grofman, *State University of New York, Stony Brook.*

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* To learn the basics of parliamentary procedure we would recommend Alice F. Sturgis, *Learning Parliamentary Procedure,* McGraw-Hill, 1953. Her *Standard Code of Parliamentary Procedure* is also excellent.

The Origins of Socialism. By George Lichtheim. (New York: Frederick A. Praeger, 1969. Pp. 302. $6.95.)

The purpose of Professor Lichtheim's book is not merely what it seems to be. The purpose is to "clarify the origins of socialism, both as a worldview and as the specific response of workers and intellectuals to the twofold upheaval of the French Revolution and the industrial revolution." (p. vii) This is achieved by presenting socialism as originating in French and English thought but receiving its "classical formulation" in Marx "with the help of German philosophy." This theme explains the tripartite division of the book, entitled respectively "Heirs of the French Revolution," "Critics of the Industrial Revolution," and "German Socialism." The author himself, however, renders this explanation questionable. Although "socialism's greatest thinker" did his "real theoretical work as an economist" after 1848, "technical reasons" forced concluding the book with that date. This strange omission suggests that the "technical reasons" in question are of more than passing importance.

Even without this problem, the book's structure is "peculiar and somewhat daunting" since "German Socialism" is not congruent with the other titles. His explanation: the book was originally intended as "the opening section of a general history of socialism and communism." The author does not say why its structure was not properly adjusted, since that would require changing only a title. This omission raises the question why he broaches the structure at all? And why is either problem relevant to the reviewer? The answers have to do with what the reviewer understands to be a conclusion reached by the author in the course of his "general history."

sons" forced concluding the book with that date.

Although there are no further statements as to why the original plan was not completed, there is another reference to "technical points" (p. viii). These have "for the most part" (i.e. not completely) been relegated to the Notes. The suspicion that the Notes have some unusual relevance is supported by the author (p. ix): "The discerning reader who takes the trouble to consult the Notes. . . ." However, the Notes' dimensions have "been swelled by the attempt to cite as many sources as possible." This reason explains neither their size nor their importance for the "discerning reader."

What does the author mean by such peculiarities explained by weak reasons? He disclaims "any intention beyond the obvious one of providing *the student* with a concise analysis of the subject and a critical introduction to the literature" (p. viii). But "the discerning reader," not "the student" is told to consult the Notes. The discerning reader notices the proportion (1:3) between Notes and text. The number 3 recalls the "peculiar and somewhat daunting" external arrangement. Part 3 merits peculiar attention especially since the author said (p. 98) Part 3 is "The Marxian Synthesis" rather than "German Socialism."

"The Marxian Synthesis" asks whether "the threshold of our concluding chapter" coincides with "the summit of our theme?" The answer "depends on what one expects from an analytical account of socialist origins." The answer never becomes explicit. But we can glean an answer which explains the "technical reasons" which puzzle this reviewer.

The original theme must be qualified. Marx was not merely a socialist writer who "combined German philosophy with British economics and French socialism." He was a philosopher essentially concerned with "the genesis and functioning of modern society." By 1848 Marx transcended socialism as such. This synthesis may "continue to be of importance to the historian of philosphy long after socialism has taken its place, with liberalism and conservatism, as one particular reaction to the twofold upheaval of the industrial revolution and the French Revolution." And having done that Marx abandoned philosophy.

I suggest the major technical reason for ending this study with 1848 is the author's conclusion that at socialism's "summit" (the subject of his concluding chapter) "Marxism" chose "critical" (partial) understanding, which could be revolutionary, over "comprehensive" understanding which it thought must necessarily be static. Philosophically, as distinguished from economically or politically, that is a decline. The historian of philosophy cannot but be daunted by the moral and intellectual price of this choice and it is not surprising that the "external arrangement" of his book should bear marks of his dismay.

Students will find this book intimidating because it is written with extraordinary care and because the attention devoted to unfamiliar figures and to explicating complex movements is overwhelming. Historians of philosophy should brush up on political economy before reading Part II. Conceptually, the author's link between socialism and Natural Law reveals a defective understanding of the latter's development. He speaks of "*the* ancient doctrine of Natural Law" (p. 107, italics mine) which begins with the Stoics and has merely a "variant" in Locke and his followers. Seen as one unbroken tradition, he cannot explain how it gave rise both to inequality and private property, and to equality and common ownership (pp. 108–109). This problem can be explained by recognizing two traditions, *jus naturale* and *lex naturalis*. Failure to distinguish them was already a Hobbesian theme. (Leviathan, Chapter xiv). On the whole, this book is recommended to those seeking a comprehensive, critical, and non-doctrinaire view of the topic. Students seeking instruction about particular figures or movements should approach it with considerable background.—GARY D. GLENN, *Northern Illinois University*.

AMERICAN GOVERNMENT AND POLITICS

Public Officials and the Press. BY DELMER D. DUNN. (Reading, Mass.: Addison-Wesley Publishing Company, 1969. Pp. 208. $2.95.)

This study undertakes the task of adding to our knowledge of the interrelationships of news reporters and government officials through as intensive analysis of the views and activities which make up these relationships in a particular environment—state government. Conceived in the image of Bernard Cohen's *The Press and Foreign Policy* it adapts Cohen's conceptual framework and applies it at the state level, going beyond Cohen (and other studies of national news reporting) by specifically focusing on the differences in press relations found among executive officials, administrators and legislative leaders. An effort also is made to assess the impact of the particular level of government.

Data for the study are drawn primarily from interviews with 50 state officials and 21 political reporters in Wisconsin in the winter of 1966–67. These were supplemented by personal observation and newspaper reading. Three brief Appendices provide details on methodology, conceptual framework and related research.

The author meticulously describes the work of gathering and presenting the news and the conceptions reporters have of their roles. This is followed by a presentation of the public official's side of the picture—his view of the reporter's role and performance, how much he is exposed to the press, what he gets from the press and how he goes about providing information to the press.

The book provides no startling conclusions. The author finds his data tend to confirm the principal judgments reached in previous studies at the national level by Cohen, Dan Nimmo, Douglass Cater and Donald Matthews. It can be said, also, that his data underwrite impressions this reviewer has acquired in some years of observing state capital political reporting—impressions, I suspect, that are shared by many political scientists.

Even though Professor Dunn finds "press relations are closely akin at both levels of government" (p. 150) he notes some distinctions. There is less need for the press to help communications in state government than in the Washington bureaucracy. State officials do not need the instant reporting of news tickers—life in the states is not crisis-oriented. Nor do they have as great a need for the structured interaction of the press conference. Such differences are more likely related to government size than to government level, the author suggests. The thing he sees which sets apart all state governments—big or small—from the national government in press relations is the lack of public interest in state affairs. This lessens coverage, reporter interest, and officials' opportunities to obtain publicity.

The principal finding of the study is that different offices have different relations with the press, depending upon the officeholder's views of the press, his information needs, and his purposes in dealing with reporters. What constitutes "news" is still crucial, and some officeholders and certain activities are more likely than others to meet these vague criteria.

The Governor has a decided advantage in access to reporters and ability to use them to his advantage. Legislators are found to have more intimate ties with reporters, stemming from greater need for the press as an information source and the "newsworthy" nature of legislative activity. Administrators are least likely to develop close relations with members of the press, even though they find them valuable communication aids and are less wary of reporters than are elected officials.

These are but a few of the specific findings of this study. They are carefully enumerated throughout the book and recapitulated in the concluding sections. The author makes no claim to complete delineation of the way the press affects policy-making in state government, but he ends by suggesting its impact will be the result of its attention-focusing ability, information-conveying and reporting on performance. The impact will be maximized, he claims, when policies are new or changing and when policy-makers are inexperienced.

The writing style utilized in this book does not arouse much reader interest. It is a precisely arranged, methodically presented body of data, each chapter neatly subdivided with major points listed initially and then elaborated upon—all in 1, 2, 3 order. The format ultimately made this reader feel the same message was being repeated over and over again—that limited data were being stretched to the utmost. Occasionally this seems to be the case, as when an important-looking italicized finding that "administrators, more than other officials, use the press to gauge timing" (p. 107) turns out to be based on the fact that there were responses in this vein from 4 of 25 administrators and no such responses from the 20 elected officials being covered. There are times when it is unclear whether or not the author has gone beyond his data in making generalizations. In part these difficulties arise because the author dissects his data from every point of possible interest.

Despite these weaknesses, the study does what the author claims. It adds to our limited supply of systematically-gathered knowledge about the relationships of public officials and news reporters. It broadens our understanding of statehouse politics. It provides a carefully-drawn analysis which invites replication in other states. In an age of mass communications and professional public relations, these constitute worthy contributions.—EARL A. NEHRING, *The University of Kansas.*

Metropolitan Decision Processes. BY MORRIS DAVIS AND MARVIN G. WEINBAUM. Chicago: Rand McNally & Co., 1969. Pp. 150. $6.00.)

Davis and Weinbaum have undertaken a controversial task and are to be congratulated, first off, for having the imagination to do it. They have assembled thirty-two rather diverse case studies of policy-making in metropolitan areas and attempted to systematize the case data by coding the narratives according to each interaction reported or clearly implied. After making certain adjustments—the authors are impeccable in reporting their procedures in detail—2,842 interaction units are available for analysis.

Seven aspects of interaction are employed as variables: *modes,* distinguishing among meetings, messages and public statements; *actor roles; spatial location,* including central city and three levels of remoteness from the city; *permeability,* or the extent to which the interaction was open to observation or participation; *formality* or structuredness of the interaction; continuity, or the extent to which the interactions are *ad hoc* and ephemeral, extended but specific to the case, or regular community patterns; and *flows over time.* The time variable is analyzed primarily by dividing the total period of the case-event into four equal periods and observing interaction rates and associated factors in the quartile periods.

The legitimacy of the Davis-Weinbaum undertaking can be considered from at least two perspectives. One is pragmatic—does it generate theoretically meaningful empirical patterns? Secondly, we must ask whether we can take seriously any results derived from procedures which, by systematizing the unsystematic impressions and interpretations of diverse authors, are so far removed from the empirical reality. To this reviewer the answer to both questions is a qualified affirmative. There are certainly some interesting findings. For example, the authors discover that business interests participate at a rather steady and high rate throughout the life of a civic issue while non-business groups tend to come in mainly toward the end. Or, again, small cities display a more closed and informal pattern of interaction than large cities. There are several dozen propositions derived and conveniently summarized at the end of each chapter. Still, one has a sense of disappointment at the final product. Not many of the tables generate much grist for an interpretive or theorizing mill. Relatively few of the propositions are likely to provoke new directions in research.

Perhaps the difficulty lies in the procedures employed. Do case studies—at least, these case studies—contain rich enough ore to make mining worthwhile? I believe that Davis and Weinbaum have demonstrated techniques by which the ore can be recovered, but I doubt that it is as yet an economical undertaking. If we contrast this effort with James Coleman's synthesis, *Community Conflict* (1957), a significant difference is apparent. Coleman pulls together a number of studies into his impressionistic bag, ponders them, and then generates some hypotheses which may *subsequently* be tested systematically by others. Davis and Weinbaum are trying to reverse this process, developing theory out of a rather special and quite limited kind of research—the narrative case study—which has been done already and done usually for heuristic or pedagogical purposes rather than to contribute to the development of empirical theory. Most cases, taken by themselves, seem to me rather low grade ore. Lest we seem more negative than is intended, however, it should be stressed that for mining case study materials Messrs. Davis and Weinbaum have provided an impressive model, and they have developed enough usable gold to encourage others to make comparable efforts with other sets of cases.—ROBERT H. SALISBURY, *Washington University.*

Congress and the Public Trust, Report of the Association of the Bar of the City of New York Special Committee on Congressional Ethics. BY JAMES C. KIRBY, JR. (New York: Atheneum, 1970. Pp. 351. $8.95).

This volume, brought out by a committee of the prestigious New York City Bar Association is a timely, if limited, contribution to the recent discussion and debate about Congressional ethics and, even more broadly, Congressional reform In a sense, it is a companion volume to an earlier report by the Association on conflict of interest and the executive branch. As a result of its studies the Committee proposes: 1) a Model Code of Conduct 2) Model Disclosure Rules 3) some recommendations for changes in existing statutes, such as in the area of campaign financing and finally 4) some informal standards for Congressional conduct, such as total abstention from the practice of law.

In arriving at these recommendations, the Committee pursued an extensive research program including a review of reference materials, discussions with key observers (journalists, attorneys, Congressional staff, administration and social scientists) and extensive interviews with a stratified sample of Congressmen. Its focus was on the following topics: conflicts of interest, Congressional law practices, campaign financing, salaries and allowances, gifts, supplemental funds and honoraria.

The report is noteworthy in several respects. First, its lack of self-righteous finger pointing. Rather it attempts to understand the behavior of Congressmen in the milieu within which they work (particularly its special pressures and demands), and the nature of the functions they perform in the political system. Second, they never underestimate the complexity of the topic, nor the impossibility of entirely wiping out the ambiguities in any set of standards. And they recognize that no code can ever replace an ultimate reliance on the decency and integrity of the members themselves and the sanction of the ballot box. Nor do they regard their proposals as a panacea or final answer in a society with changing values and sensitivities.

The Committee, however, narrowed its focus, perhaps unduly, and as a result did not consider some very timely and crucial topics. For example, they excluded intervention by members of Congress into issues pending before administrative agencies. Such intervention raises some very difficult and perplexing questions and is deserving of study. They also excluded lobbyists on the grounds that while their methods raise ethical questions, it involves the ethics of constituents and interest groups more than the ethics of members of Congress. This seems a rather debatable compartmentalization of the legislative system.

In its conclusions, the Commiteee also seems highly optimistic, for they contend that "Congress has entered an historic new phase of ethical self-discipline." (p. 231) What this conclusion is based on is not made clear. However, it seems related at least in part to the establishment in each chamber of an Ethics Committee. But the study suggests earlier that such Committees would not have been embraced by Congress without the impetus of the Baker, Powell and Dodd cases and the pressure of public opinion and that the reluctance to use self-discipline is still in the background (pp. 220–21). Indeed, what groups (whether it be teachers, policemen, lawyers, doctors, etc.) do discipline their fellow members except under outside pressures of considerable magnitude? Perhaps, it would be more realistic to include public members on such Committees or set up an outside Commission to hear complaints, as was done with the salary issue, with each chamber reserving final power.

Because this is largely a set of prescriptions for behavior the dynamics of Congressional life is somewhat lacking. This is particularly evident in their five page survey of the history of the Senate and House Ethics Committees (pp. 216–221) which is notably flat and uninformative. This reviewer found James Boyd's *Above the Law* or Robert Getz's *Congressional Ethics* more interesting in this regard.

Finally, as the authors are well aware, they are dealing with only a small facet of Congressional activity that has bearing on the public's perception of the integrity and responsiveness of Congress. Indeed, it may not even be the most important one. The rules and procedures, the openness of the institution, and its responses to current problems and challenges may be even more pertinent in an appraisal of Congress as a responsible institution.—DALE VINYARD, *Wayne State University.*

The Politics of Corruption: Organized Crime in an American City. BY JOHN A. GARDINER. (New York: Russell Sage Foundation, 1970. Pp. 129. $5.95.)

John A. Gardiner's book is well worth reading, albeit quickly. This monograph is a case study on the relationship of organized crime to civic corruption in "Wincanton U.S.A." (The names appear to be changed to protect the guilty.)

This work was developed out of research done for the President's Commission on Law Enforcement and the Administration of Justice, and suffers accordingly. Like many reports padded into books, this is a "deja vu" book, i.e. thoughts, ideas, summaries, even whole sentences appear and reappear like bit players in a "cheap-jack" movie. If this was a dissertation evaluation one would recommend that there are a couple of *good articles* here and no more. Thus, though a short work, it merits the "quick reading" designation.

In spite of this I recommend the book. In fact it would make a good supplementary reading in any undergraduate urban problems or policy course having a section on crime. The merit of the book is that the author has stressed and reminded us—in our recently rediscovered emphasis on organized crime and police policy—that civic corruption is the societial payoff of organized crime, and has illuminated its operation. The effects of civic corruption are detailed—some $2,400 a week was pocketed by Wincanton's key public officials—as is the effect of corruption on the political attitudes of Wincanton's citizens. At the same time, the author successfully balances the dilemma of a democratic society between the demands for a vice-free society and the recognition of a vice tolerant human nature.

In this book Gardiner also casts light on the multifaceted nature of corruption. He shows, for example, how when top officials "sell out" those below them have few alternatives—if they want to stay around—but to keep quiet or join in. He also notes the effects on "beat officers" who, aware of the corruption of their superiors, are tempted to engage in their own petty rackets, such as accepting bribes from speeders. Perhaps most frightening, in a free society, he notes how a "bought government and police" can be used to "legally" remove those who do not accept the leadership of the organization.

Not one to back off from his research, Gardiner offers, in conclusion, some concrete suggestions about how the corruption of law enforcement agencies might be reduced.

First, improve the internal structure of law enforcement agencies. In Wincanton, as in some Connecticut cities, party loyalty, patronage, and payoff work to constrain and defeat merit and dedication in the appointment and operation of the police department.

Second, improve the role and initiatory energies of state and federal agencies. The Wincanton case suggests that these agencies are at times remiss in developing and initiating legal action when that action might intrude on the local police agency even where there is a strong smell of official corruption, and that petty rivalries between agency bureaucracies at times hinder and delay this action.

Third, eliminate the fragmentation of local government and the decentralization of power that facilitates the division and conquest of local authorities (Wincanton possessed a modified Commission system). To this end the author suggests a strong mayor system and notes the virtues of democratic centralism. While not convinced on this point, since it would seem that any structural form is corruptible, one must defer to the author's experience.

Fourth, attempt to reduce the conflict between popular values and formal legislation. Here the author notes that the public is inadequately aware of the relationship between the consumption of illicit goods and services and the growth of organized crime and corruption. He suggests that while laws exist, public ignorance, apathy to "crimes without victims," and human interest in vice, may make some middle path most acceptable. Certain forms of vice, notably gambling, should perhaps be legalized, or perhaps the government should negotiate a series of "understandings" or license the purveyors of vice. An interesting sidelight to this point, the author finds that in Wincanton, as elsewhere, one-issue reform movements that come in to "clean things up" fade in the stretch and are a poor return bet.

Organized crime in America is a big business. In fact it is one of America's largest enterprises. In Wincanton, horse and numbers gambling grossed $1.3 million annually, the dice game had an *employee payroll* of $350,000, and the illegal still could produce $4 million worth of alcohol a year. One notes that in comparative terms this is a relatively small operation, but then "Wincanton" is a city of less than 100,000 people. Unfortunately, few Americans realize that the operations of organized crime stretch far beyond our major cities to gross, by the best government guessestimate, some $7 to $50 billion a year from gambling alone. Fewer still realize how it can corrupt government and law enforcement to the point where all citizens suffer and civic trust is eroded. By bringing these problems to light Gardiner has performed an important service, and his information and insights should be passed on to students and citizens alike.
—Peter A. Lupsha, *Yale University.*

Aging and Society. Volume 1: An Inventory of Research Findings. Edited by Matilda White Riley, Anne Foner and Associates. *Volume 2: Aging and the Professions.* Edited by Matilda White Riley, John W. Riley, Jr., Marilyn E. Johnson and Associates. (New York: Russell Sage Foundation. Vol. 1, 1968. Pp. 636, $35.00; Vol. 2, 1969, Pp. 410, $9.50.)

These two first volumes of a trilogy sponsored by the Russell Sage Foundation tell a great deal about aging, even more about "OP" (older persons), much less about aging *and* society, and virtually nothing about society. This is too bad. For the effort and the conscientious care which have gone into the fashioning of these volumes have clearly been very great. A platoon of 68 distinguished "consultants and advisors" has been mobilized to conduct painstaking search missions through the many chapters of facts, figures and interpretations. The failure of the work to come to grips with social processes and problems—let alone political ones—is all the more disappointing. Possibly volume three, which is to examine the "implications (of the research findings) for sociological understanding" will remedy this defect; but the chance seems remote.

It is difficult to find the proper term to describe Volume One, the "Inventory." The words "atlas," "compendium," and "propositional inventory" all come to mind. Innumerable research findings on the economic, social and physical condition of the aged have been culled from virtually every respectable study done in the United States and from a few conducted in other countries, mostly those of Western Europe. Each finding is summarized succinctly in a few precise sentences. The multitude of findings is presented in tightly organized chapters, so that it is possible to examine at a glance all of the research results bearing on some particular question. The most important or most typical findings are illustrated by "exhibits"—charts and graphs which convey information with striking clarity and economy in presentation. The editors, moreover, show much scholarly concern for the quality of the information. They have selected only studies which, in their judgment, have been competently executed; and occasionally they add cautionary notes apprizing the reader of possible errors due to research design problems or sampling biases.

All of this is most commendable. Irrespective of its content, the volume is a monument to scholarly thoroughness and responsibility. It should be shown to every graduate student as an object lesson in the art of organizing and weighing information.

Volume Two consists of chapters written by "professionals" drawn chiefly from the faculties of professional schools. The task assigned to the writers was to derive, from the findings arrayed in Volume One, implications for the professions they represent—social work, medicine, public health, architecture, law, economics, religion, education and communications. The result is less than a full success. The professionals, aided by their advisors, essentially deal with current changes in their professions' outlook and with some special problems that older people present to them. Professor of Medicine Louis Lasagna and Professor of Nursing Doris R. Schwartz each devote long sections of their chapters to the ethical and practical problems of care for dying patients. On the other hand, Professor of Economics Juanita M. Kreps presents a careful analysis of income, expenditures and savings among age groups as a foundation for a rather inconclusive discussion of income re-distribution. Professor of Architecture John Madge speculates about the desirability of various housing arrangements for older persons. Whether or not they quote Volume One, it is rather doubtful that the findings have in any important way affected their views. Indeed, in spite of the editors' insistent prefatory notes on "the relevance of Volume One," it is rather difficult to see by what logical procedure the "professional" could have drawn policy conclusions from the facts contained in the first volume.

Volume Two, even more than Volume One, reflects the state of the arts; and the state of the arts reflects the state of our society. We tend to see social problems as the problems of some particular group—the young, the old, the black, the poor—and not as problems inherent in our social and political structures and in our cultural practices and values. To be sure, the aged do have special problems, but many of these are different from those of other people in magnitude rather than in kind. Old people are sick more often and more severely than are younger people, they are (perhaps) more isolated, they often are severely deprived by insufficient incomes. But the problems that cry for both scholarly analysis and social action are those of health care, social integration and income maintenance for *all* people. The physical lay-out and the social climate of our cities is not merely bad for the aged (or the black, or the poor), it is just plain bad. It is rather doubtful that either systematic scientific analysis of social processes and systems or coherent policy making will be possible so long as we keep focussing so much of our attention upon some particular groups of whose existence we have become painfully aware.

In this context, it is rather significant that Volume One has virtually nothing to report about society. A few paragraphs are devoted to "norms," e.g. legislation concerning the financial responsibility of children for their aged parents. Reading these two volumes, one would never guess that the current welfare system has been accused, by both the left and the right, and by both academics and practitioners, of undermining the self-reliance and personal dignity of recipients. And, *a fortiori*, there is nothing to tell us what social and political processes make the aged (or the young, or the black) a special group in society in the first place —a group which the public, the expert and the policy maker then perceive as a problem group.

The editors and authors would clearly have been able to include much more material on social policy, minimally some compilation of legislative patterns and possibly an examination of their socio-economic corrolaries. They might have searched for more than the very sketchy material they present on public attitudes and cultural values. It is not clear, offhand, why these topics should be of less interest to the social scientists and professionals (to whom the work is addressed) than are twenty pages on collagen, brain weight, lipofuscin and other topics in the physiology of aging. Thus, it is difficult to dismiss the notion that the failure to discuss society in a work on Aging *and* Society is due to our inability to see aging as a problem *of* and *in* society, not one of the aged *vis-à-vis* society.—FRANK A. PINNER, *Michigan State University.*

Two Hundred Million Americans In Search of A Government. By E. E. SCHATTSCHNEIDER. (New York: Holt, Rinehart and Winston, Inc., 1969. Pp. 133. $2.95.)

Many of us who have long enjoyed the warm wisdom and the graceful scholarship of E. E. Schattschneider will wonder why this slim volume. Would not the same energy, devoted to an updating of *Party Government,* serve even better thousands of college students? Perhaps so, but if one has spent an evening with this essay after listening to several eighteen year olds solemnly declare that they are prepared to give the system "one more chance," its need becomes more evident. A recapitulation of the naive and self-serving transparencies of the plan to shut down the colleges two weeks before Election Day so that students and faculty may work for the "candidates of their choice" also helps.

What Prof. Schattschneider has provided is a careful, patient walk down the Sesame Street of politics. It is as though he is gently whispering the facts of political life to the acne radicals and their syncophants at the faculty club. Thus we are reminded that there are limits to what government can achieve, but that government is necessary to do those things that are always overlooked by the utopians. Stress is put on the ways that external conflict shapes domestic issues. We are reminded of the moral assumptions of democracy, but we are also cautioned about man's imperfections.

Prof. Schattschneider cautions his reader against confusing government-by-consent-of-the-governed with government-by-the people and warns against the danger of defining the system as one "in which everybody is supposed to get his way." He will undoubtedly test even the tolerance of the Sigma Chis and their sweethearts with his insistence that a generation is a proper period of time for the study of politics. His thoroughly documented insistence that "students whose political memories go back only a decade have a staggering intellectual problem attempting to understand what is happening" is written with the courage that comes with emeritus status.

The essay concludes with pleas for common sense and a recognition that those of us inside and outside of government had better recognize that since the mess we are in is intellectual we had best start thinking about what government is before we assign it impossible goals.

In order to familiarize students with the immense changes which have taken place in the past century, Prof. Schattschneider includes an Appendix that lists 500 words and phrases in our current political jargon which do not appear in an 1859 dictionary. A curious omission is, "up against the wall, professor."

Those of us who still make a living by burping undergraduate true believers are again indebted to Prof. Schattschneider. Right on, E. E.—SIDNEY WISE, *Franklin and Marshall College.*

Freedom and the Foundation: The Fund for the Republic in the Era of McCarthysim. By THOMAS C. REEVES. (New York: Alfred A. Knopf, 1969. Pp. 355. $7.95.)

In this account of the Fund for the Republic, an independent off-shoot from the Ford Foundation, Professor Reeves, an historian, aims at two overlapping, if not entirely complementary, objectives. His first intention is to review the record of this staunch action-and-research civil liberties organization by examining its turbulent encounters with such Congressional adversaries as the House Committee on Un-American Activities and the Senate Internal Security Subcommittee, in addition to the barrages of criticism from noted journalists and broadcasters of that period. Secondly, he attempts to explore the Fund's internal life and operations in order to add to the slim body of analytical studies on tax-free foundations. By and large Professor Reeves achieves the first purpose in this exhaustively researched, (he had access to the foundation's complete files and records, which is reflected in 57 pages of closely-packed notes) clearly written and sometimes eloquent chronicle. However he falls short of the second objective primarily because he does not systematically examine the Fund's internal dynamics.

Robert Maynard Hutchins, the very prototype of an academician turned foundation impresario, emerges as the key force in explaining the Fund's successes and failures. For as consultant to Paul Hoffman, the President of the Ford Foundation, Hutchins defined the objectives and prepared the guidelines for the new venture; later serving as associate director and President of the Fund for the Republic over most of its ten-year existence. It was Hutchins who determined what programs were to be undertaken and how the grants would be allocated; and finally it was his iconoclastic passion for civil liberties combined with abrasive wit, more so than the ongoing work of the Fund that unleashed the wrath of the opposition and threatened the organization's continued survival.

Only when it became evident that the Fund was in danger of losing its tax-exempt status and would thereby forfeit the entire Ford Foundation subsidy, did the board of trustees, drawn largely from the ranks of establishment lawyers and corporation executives, intervene to restrain Hutchins and erase the growing fellow-traveller image. After narrowly avoiding dismissal, Hutchins tempered his own public appearances and modified the organization's direction. Gradually, projects that had angered professional anti-communists, such as studies critical of blacklisting practices, federal loyalty-security programs and Congressional investigations were discontinued or allowed to lapse, and funds were channeled to more acceptable programs in the fields of school integration, race relations and the threat of American Communism. Moreover an extensive public relations campaign was launched to persuade Middle America that the defense of civil liberties was in the spirit of true conservatism. Whatever embarrassment was suffered by the campaign—which elicited favorable response from both J. Edgar Hoover and the American Legion—was more than offset by declining interest in the Fund on the part of Congressional investigating committees and the Internal Revenue Service.

By 1956 the fervent national mood of anti-communism had abated, and the Fund was no longer a center of great controversy. While Hutchins's crusading impulses remained intact, the absence of formidable opponents sharpened his discontent with his administrative chores. With approximatley 7 million of the original 15 million Ford money yet to be spent, Hutchins won the trustees' approval to reconstitute the Fund as the Center for the Study of Democratic Institutions in Pasadena, thereby bringing to fruition a lifelong ambition that dated back to his association with Mortimer Adler at the University of Chicago to found a Platonic Academy. But as several staff members who opposed this proposal correctly forsaw, a permanent community of scholars cut loose from their institutional bearings would be more conducive to petulant infighting than new insights into the great issues.

As a grant-giving agency the Fund scored notable achievements, having encouraged and brought to completion such enduring works as Samuel Stouffer's *Communism, Conformity and Civil Liberties,* Telford Taylor's *Grant Inquest,* Lazarsfeld and Thielen's *The Academic Mind,* and Adam Yarmolinsky's *Case Studies in Personal Security,* among others. Yet it is ironic to observe that the area where Hutchins felt the Fund could make its most distinctive contribution, the inculcation of widespread attitudes favorable to civil liberties, proved a qualified failure due to Hutchins's intemperate approach, poor choice of grantees, and resistance from the mass media and the general public.

Political scientists interested in the Fund as a case study of the problems of a nonprofit social science foundation will be disappointed with this book since the author presents no model or holds no firm conception of this type of organization to enable the most relevant data to be presented and judgments offered. Professor Reeves's fascination with the complexities of Hutchins's behavior and his desire to recapitulate by now familiar highlights of the McCarthy era tends to obscure the less obvious internal order. As a result important aspects of the Fund's operations are neglected, passed over lightly and sometimes misconstrued. For example, a comprehensive listing of the grants and recipients approved at board meetings is recorded, and the reader is assured in the conclusion that the projects were painstakingly and fairly selected. But neither the criteria or the screening process that produced the unusual record of successful work is discussed in detail. Professor Reeves affirms the wisdom of the Ford Foundation to restrict membership on the board to establishment figures, alluding to the loss of widespread support if a Roger Baldwin or Norman Thomas were to have served. Yet it is precisely such battle-hardened civil libertarian veterans who might have kept Hutchins under a tighter rein and averted the constant pitfalls that led to near extinction. Although the possible loss of tax-exempt status is a recurrent theme the author does not deal with the issue of whether the Fund's activities may have indeed violated existing laws and regulations. On at least two occasions it is noted without comment, substantial grants were made to Congressional committees sympathetic to the Fund's programs.

While the author fails to draw any parallels between the Fund for the Republic's struggles over civil liberties in the 1950's and the Ford Foundation's involvement in the juvenile delinquency, anti-poverty and community decentralization controversies of the 1960's, such a comparison would seem to be particularly appropriate for those wishing to evaluate the impact of social science foundations on public policy. Even though the current situation appears to be more complex and many liberals with strong commitments toward constitutional freedoms are ambivalent about these contemporary issues, the continuity between these approaches outweighs their differences. In point of fact the Fund may have served as a staging-ground for these newer social programs since many of its staff during the 1950's—Joseph Lyford, Michael Harrington, John Cogley, Adam Yarmolinsky and Paul Jacobs have been closely identified with the programs emerging in the New Frontier and Great Society. Since the days of free-wheeling foundations are about numbered, and the files of the Fund are conveniently on deposit at the Princeton University Library, a re-examination of the Fund from this perspective might prove to be highly instructive.—ARNOLD J. BORNFRIEND, *Queens College, City University of New York.*

The Influence of Federal Grants: Public Assistance in Massachusetts. BY MARTHA DERTHICK. (Cambridge: Harvard University Press, 1970. Pp. 285. $8.50.)

This book is a descriptive case study of the impacts of federal grants for public assistance in Massachusetts from the 1930's through a major reorganization and centralization battle in the late 1960's. As a single-state analysis it reflects the assets as well as the limitations of the case study method.

This is not a standard chronological, blow-by-blow account of events, however. The bulk of the book (Part II, "The Pursuit of Federal Goals" extracts five issues for longitudinal and critical analysis. The five are: (1) adequacy of assistance, (2) equity of administration, (3) efficiency of administration, (4) giving of services, and (5) professionalization of personnel. This descriptive-analytic core is preceded by an introductory segment, "The Federal Government as Grant Giver." It provides an overview of the federal grant "system" and of the public assistance function. Part III, "The Consequences of Federal Action," discusses the exercise of federal influence and prospects for the grant system.

Among the themes and conclusions constituting a capsule summary of the book, I would offer the following:

(1) federal influence, in addition to setting the agenda for the Massachusetts legislature on welfare issues, produced centralizing and professionalizing results;
(2) influence exercised by federal administrators, to be effective, must be employed discretely and preferably subliminally;
(3) a high level of conflict between federal and state welfare administrators may exist simultaneously with a high degree of cooperation and mutual support;
(4) there is considerable utility in national administrators emitting ambiguous stimuli in an effort to precipitate state action;
(5) intergovernmental relationships are cut from the same whole cloth from which diplomatic relations are derived, namely, conflict bargaining, collaboration, coalition formation, negotiation, accommodation, risk, uncertainty, and strategy;
(6) welfare, as Gilbert Steiner argues, is an issue transcending partisan politics;
(7) it is difficult and perhaps impossible, as well as undesirable, to specify the conditions under which federal officials can measure with precision the degree to which states conform to federal standards:
(8) after introductory innovations, intergovernmental relations operate incrementally (perhaps glacially) in the welfare field, with more than three decades required to achieve the structures, processes, and attitudes at the state-local level that closely approximate the objectives of national administrators;
(9) intergovernmental relations are intimately linked to the more general features and philosophy of the American political system, particularly pluralism.

Criticisms of the book are best expressed by suggesting what the volume does not cover.

First, no careful or explicit theory or conceptual definition of influence is presented despite the statement (p. 5) that "My central purpose is to analyze the grant-in-aid system as an instrument of federal influence." The facts do, indeed, reveal that influence was exercised but the reader is left with considerable doubt about the generic character and the asymmetric aspects of the federal-state-local relationships described. Federal influence finally brought about centralized, state welfare administration in 1967. But can the actions taken and results achieved from this use of influence provide a basis for comparison in other areas of intergovernmental relations? Can the thirty years required to accomplish this result be traced to "weak" federal influence or "strong" localizing influences?

A closely-related second omission arises from the limitations of the case study method. Miss Derthick proposes "the case of public assistance as a standard of comparison and a source of hypotheses for the investigation of other programs" (p. 218). I found many important *issues* raised in the book, but I did not obtain the quasi-scientific precision conveyed by remarks about a standard for comparison (the measurement problem) and the hint at hypotheses (relations between two or more specific variables).

This volume is not a policy critique of federal grants-in-aid or of the public assistance program, although the reader is provided ample evidence to arrive at his own policy conclusions. Miss Derthick's conclusions are analytic, abstract, and twice or thrice removed from translation into policy considerations.

On at least two points the level of abstraction leads the author to state as assumptions two intergovernmental considerations that are better treated as problematic. Both are captured in a single statement (p. 7): "In short, there is a grant system—a system by which the federal government seeks to influence the conduct of state and local governments in such a way as to promote the realization of its own goals." The proliferation of categorical grants into 160 programs and over 500 authorizations calls into question either the assertion that a system exists or else the definition of the term system. The second problematic element in the quotation is the implication of a unitary, common, or underlying aim representing the motive force behind grants. Apart from the reification present in the statement, the matter is not a closed question. It has the potential for laying bare a basic issue troubling our politics today. I refer to the debate over whether there is a conscious collective, and concentrated mobilization of power (bias?) in government or whether diverse, dispersed, and pluralistic patterns of value allocations prevail in the political community.

A concluding way of describing and identifying this volume is to compare it with recent books dealing with welfare and intergovernmental relations. It lacks the sense of policy criticism present in Steiner's *Social Insecurity,* although its depth and detail furnish evidence of the political constraints under which one welfare program has operated and blunts a few of Steiner's barbs. As a single-state case study it lacks the quantitative-comparative, policy-output approach adopted by Dye, Sharkansky, *et al.* But this volume sheds much more light on the impact of politics, processes, and administration on welfare policy than any set of correlation matrices. Compared with Elazar's *American Federalism: A View From the States,* it also focuses on state government as a keystone in the federal arch but deals less with imaginative and impressionistic interpretations of state politics or on individual motivations as a basis for a state's culture. But *The Influence of Federal Grants* does detail the dynamic and systemic character of intersecting forces on state decision making from a political and administrative standpoint. It also pinpoints the effects one state's political culture has on specific policy decisions. In adopting a process approach, the volume is close intellectually to the tack taken by the late Morton Grodzins (e.g., *The American System*). Miss Derthick parts company with Grodzins, however, in not sharing his descriptive and normative views of the American system as preponderantly cooperative and collaborative, one dominated by a benign "mild chaos." Miss Derthick finds and contends that conflict is at least an equal partner in intergovernmental relations and she implies that more than a mild degree of chaos inheres in the system. It would have been interesting and instructive to have had a more direct discussion on whether the current degree of chaos is tolerable.

My overall evaluation of this book is fundamentally favorable. It combines a thoroughness bordering on the exhaustive with a judicious, economical, and clear presentation of facts and events. It weaves politics, policy, and administration in the seamless web found among these modes in the real world. It blends the description of issues with a healthy flavoring of political insights (sometimes buried in footnotes). The book should serve as a valuable supplement for political science courses in public administration, intergovernmental relations, and public policy and also for professional courses in welfare administration. The contents can be easily grasped by advanced undergraduates. —DEIL S. WRIGHT, *University of North Carolina.*

Taxes and Politics: A Study of Illinois Public Finance. BY GLENN W. FISHER. (Urbana: University of Illinois Press, 1969. Pp. 332. $10.00.)

Many recent political science studies of state taxing and spending policies, such as those by Dye and Sharkansky, have been broadly comparative in approach and have involved substantial statistical analysis of the relationships between political and economic variables of various sorts and public policy. In marked contrast to this genre, Professor Fisher provides us with a detailed examination of the experience of a single state, although he does occasionally compare Illinois with the other states on such matters as tax yields and tax burdens.

Following a short introductory chapter in which he sets the context for his study, Professor Fisher proceeds to a description of Illinois state expenditure patterns and revenue sources. His treatment of the state's general sales tax is especially penetrating and meaty. From there he moves to a discussion of local government expenditure and revenue patterns, which he relates to state activity. Especially worthy of comment here is his consideration of the nature and problems of property taxation. The next two chapters comprise the more analytical portion of the book, involving as they do an economic analysis of the burden of Illinois taxation (he concludes that Illinois is a low tax burden state) and a somewhat traditional but nonetheless insightful analysis of the politics of taxation in the state legislative arena. The final chapter attempts an appraisal of the Illinois tax system from several perspectives found in political science and economic literature, e.g., the Downsian model democracy. As this brief summary indicates, Fisher is primarily concerned with the state tax system. He does not deal systematically with the appropriations process, there really being no need to do so given the excellent earlier study by Thomas Anton, *The Politics of State Expenditure in Illinois.*

Professor Fisher approaches his task in the manner of the political economist (albeit not the "new" political economist), utilizing both political and economic modes of analysis and data. Taxation and expenditure decisions are viewed as political decisions made within a framework provided by the state constitutions, case law, and custom, and shaped partly by economic analysis and considerations. No over-arching theory guides the study, a fact which some may lament. However, the author appears quite correct when he states that "there exists no integrated theory of the economics and politics of public finance which would serve as a framework for analyzing Illinois public finance."

Much of the tax data presented, and there is a substantial quantity of it in the text and in a sizeable appendix, is for the 1942–1965 period. Consequently, one gets a good longitudinal view, as well as a contemporary cross-sectional view, of tax patterns and developments in Illinois. Most of the data is presented in easily deciphered charts and tables. Although some statistical (correlational) analysis is employed, this is kept to a minimum. Those of our brethren with statistical antipathies will likely find this agreeable while those with stronger statistical proclivities will be less enthused. Partly for this reason the volume does little to make up for the lack of an "integrated theory" remarked on in the preceding paragraph of this review.

Political scientists will find the chapters concerned with the politics of taxation and appraisal of tax system the most interesting and pertinent to their tastes. However, useful pieces of political information and tantalizing insights and generalizations are scattered throughout the work. Thus we find that the Illinois Supreme Court, through interpretation and application of the detailed provisions of the state constitution, has played a major role in shaping the state revenue system. For instance, the state sales tax for several decades took the guise of a retailer's occupation tax, a legal fiction necessary to circumvent a constitutional barrier created by a court decision. Generally, one can conclude that tax policy formation has been characterized by a fair amount of judicial supremacy in Illinois. As Fisher states (p. 243), "Many debates about Illinois taxes turn into debates about wording the proposal to make it constitutional rather than remaining debates about issues of equity, administrative feasibility, or even yield." He further suggests that this may help ameliorate tax conflicts by shifting them into verbal and technical channels. The proposition is indeed an interesting one.

Overall, this book is rich in factual content, essentially traditional in approach, competent in execution, and straightforward in style. It will be of particular interest to students of Illinois politics, specialists in state and local politics, and devotees of public finance. Some will probably wonder, upon reading or examining the book, whether in the fashion of the small boy's book on penguins it does not tell us more than we really need to know about taxation in Illinois. On the whole I think not. It is a solid reference work, among other things, and similar studies of taxation in other states would be desirable, both for the basic information they present and as the basis for more theoretically sophisticated analyses.

In conclusion, a reading of Fisher's book will provide one with much information on public finance in Illinois. It will also enhance one's understanding of and "intuitive feel" for Illinois politics, once aptly described by Fenton as the "politics of clout." A book which does these things is a worthy contribution to the literature.—JAMES E. ANDERSON, *University of Houston.*

The Public Vocational University: Captive Knowledge and Public Power. BY EDGAR LITT. (New York: Holt, Rinehart and Winston, Inc., 1969. Pp. 159. $3.95, paperback.)

The title of Professor Litt's book—The Public Vocational University; Captive Knowledge and Public Power—reveals its major thesis. The university to which he refers is the large state university and its captivity is brought on by its dependence on the Federal Government for grants and direction. His thesis is that higher education has become a "captive domain" gathering vocational knowledge which is used for political ends. Due to this captivity the university concentrates on the teaching of technical, impersonal knowledge rather than the cultivation of the inquisitive mind. Instead of being the state's critic, it treats the student to a "chauvinistic glorification of the national state." The products of this education are passive students skillful in applying knowledge to the technostructure and ready to assimilate quickly into it, rather than creative, truly intellectual and politically active human beings. The faculty, in turn, is less oriented towards dialogue between "adults and students" or inquiry with intellectuals and reformers and "more concerned about political relations with men of power." (p. 9) He compares the public university to the small liberal arts college—Amherst and Oberlin are cited—with its emphasis on humanistic studies and finds the university wanting.

This deficiency is seen as all the more alarming because large proportions of the students at the university are the children of the urban, immigrant working or lower middle class. For them the current curriculum tends to promote parochialism and to retard capacity for personal growth and intellectual criticism. In lieu of the establishment-oriented politics of consensus, which to him is apolitical, Litt would like to see the students become more politicized with a capacity for questioning the premises as well as the practice of many of our political institutions. In this context he makes some intriguing suggestions for engaging students in community action and for increasing their contacts with non-college groups to bring about a fresh new look at politics.

His major attack on the vocational university is levied against its "practical" curriculum which he judges to be inhibitory of intellectual growth and human development. Instead of the "practical curriculum," he suggests the introduction of an updated, relevant, but genuinely liberal arts curriculum with heavy emphasis on the humanities and social sciences. In this context he advocates an imaginative revision of the core curriculum and opportunity for ample experimentation in content and teaching methodology. He develops in some detail his notion of an experiental or human development curriculum. The developmental curriculum is seen as potentially liberating for the human personality, conducive to self-awareness, the shedding of old values, and the making of new decisions. It stresses intellectual eminence and increased political participation, participation which is change-rather than status quo-oriented.

Professor Litt's book can be admired for its educational prescriptions and yet be criticized for its diagnosis and its practicality. To deal with the diagnosis first: His exposé of the ills which beset the large universities is to-the-point although these same "ills" beset the best private universities since utilization of federal funds is as high there, in some instances higher, than in the public ones, and since training of personnel for the technocracy, the corporate world, and the professions is as much part of their curriculum as it is in the public university. The book can also be criticized for frequently failing to supply evidence for a major and otherwise cogent point. For example, what is the evidence that vocationalism and acceptance of grants has lead to increased service to the state when compared to the universities of yesterday? The main critique, however, must be reserved for the practicality of his plan. It is unlikely that any university, public or private, can really model itself educationally after Amherst and Oberlin not only because—as he points out—they have different student bodies but because the small college generally has no graduate school and is essentially dedicated to teaching. The university is designed as a research institution and a training institution for professionals; the emphasis on research and on technical skills therefore is all but unavoidable. Nor can we really seriously plan for such an education when our goal will have to be to admit more students than ever before—students who, because of their background, have no choice but to be vocationally oriented.

The author's preference for a liberal arts curriculum dedicated to the cultivation of the human intellect and its critical functions strikes a very responsive chord with this reviewer. Like the author, the reviewer feels that this is what education should be all about. Such education is even more needed today than it was 50 years ago because the universities are becoming host to ever larger number of students for whom this will be the only exposure they will ever have to such a cultural tradition. Having said this much the reviewer, however, hastily has to add that what ideally ought to be does not seem to be synonymous with what realistically can be. Neither students nor faculties nor taxpayers seem at the moment prepared to repeat on a large scale the experience of the small liberal arts college. Students came to college either in search of practical training or in search of relevance. In neither case do they express but scant interest in the more analytic, reflective courses dealing with literature, the historical past, or theoretical issues. Nor would they welcome renewed emphasis on course requirements—even in the name of a core curriculum. Taxpayers, on the other hand, are becoming increasingly more reluctant to finance the small classes, experimental curricula, light teaching loads, etc. which go along with, his type of quality education. The demands for open admission only compound the dilemma. The faculty on the other hand has long recognized that the greatest returns in the profession come as a result of publications and not from the number of undergraduates they educate well. Moreover, while they probably share with Professor Litt his preference for a critical and politically engaged student body, many may of late have come to look upon it as a mixed blessing and one not always conducive to the pursuit of academic excellence and serenity. All of which can spell only one thing, namely larger classes, more instruction by teaching assistants, poorer student advising, and lower standards for admission.

In short, while the Litt formula is appealing, the temper of the times and the very real need for mass education probably will limit its usefulness. There is nothing wrong with the Litt formula provided there were enough money and innovative spirit available to try it. Money and innovative spirit, however, are in short supply these days in academia.—Roberta S. Sigel, *State University of New York at Buffalo.*

The Air Force Plans For Peace 1943–1945. By Perry McCoy Smith. (Baltimore: The Johns Hopkins Press, 1970. Pp. 132. $5.95.)

This is a study of what could have been a most significant round of military decision-making, but probably wasn't. Smith's analysis covers the planning of two committees nominally entrusted with post-war planning for the Air Force from 1943–1945. We are tempted to take the deliberations of the Post War Division (of the Air Staff, Plans) and the Special Project Office seriously by default, if no one else held an explicit mandate to plan for what became the most significant military force of the post-World War II world. Yet Smith shows the produce of these committees to be relatively unrelated to this future world, except for the major structural accomplishment that the Air Force won its independence from the U.S. Army. Analyses almost never speculated about the Soviet Union as an enemy; was this because even the military was caught up in the naive assumption of close post-war relations with Stalin's country? And what does it do to the revisionist interpretations which credit the United States with launching the Cold War early in 1945, primarily in brandishing air power?

Smith argues that Air Force officers could not take the Soviet Union seriously as an enemy simply because the Russians did not yet have any strategic air force. Yet this at least suggests that no one in the White House or the State Department bothered to label Moscow as the likely enemy, if war scenarios could be shaped by such narrow professionalism. The Air Force planning committees were indeed not even privy to the atomic bomb. Perhaps this was legitimated by uncertainty as late as 1944 on whether the bomb would work, but how meaningful could planning be in the summer of 1945 without this information? Air Force officers, to be sure, could assume victory through air power in the future even with simple high-explosive bombs, but wouldn't a knowledge of nuclear weaponry have made all such speculation a little more reasonable? These planners thus had no clue that Japan might surrender in 1945 rather than 1946 and 1947. Could anyone have been attaching importance to these committees if he could not fill them in on such basic premises?

Smith never really makes clear how seriously he himself takes the role of these committees. It is difficult to write a Doctoral Dissertation and book without implicitly premising that the objects of study are functionally very significant. Some of Smith's sentences thus suggest great importance, others much less, for the "decisions" generated.

To assess the significance of these committees accurately, some questions could have been raised which this book does not answer. What about the U.S. Strategic Bombing Survey, which is mentioned only once in passing in the entire book? Was there any exchange or interchange between it and the Air Force planning here described, Some coordination might have to be present to prove that this exercise was being taken seriously. What about General Lemay's change of bombing tactics in 1945 over Japan, from the nominally anti-capability precision bombing which the A.A.F. had conducted in Europe, to a sort of counter-morale area-bombing campaign resembling that of the R.A.F.? What about post-war structuring of the independent U.S. Air Force, Does it show signs of being determined by the plans of 1943 to 1945? General Orval Anderson is briefly described as having been a functional forerunner of the planning bodies as late as 1943. Anderson, unlike his successors, was willing to point by name to the USSR as a potential enemy for the postwar world. Yet it hardly is probable that Anderson engaged in no more of this activity after 1943, or indeed after 1945. Are we thus to exclude him from discussions of Air Force planning for peace in the 1943–1945 period, and thus keep the planning records clean of any preventative war talk?

Smith encounters (and acknowledges) a familiar problem in doctrinal history, namely, whether the written analyses really reflect what seemed plausible at the time. Generals may indeed have been reluctant to write down the name of Russia as the potential enemy, and yet have generally understood it. Air Force theorists may have learned a lot from Douhet, or a little; Douhet's writings to be sure were available in translation, and thus could serve as a convenient base line from which to depart in any strategic speculation. Yet what about the experiences of the RAF, communicated verbally or on paper? And if one uses Douhet as a crutch for lecture outlines, does this really make that author more influential than anyone else? Smith has reinforced his access to documents by a good selection of interviews, explicitly recognizing that memory after twenty years can be as misleading as twenty-year old paperwork.

One can too easily dismiss "future of the universe" studies as meaningless exercises upon which good talent is wasted as tributes to the false god of rationality. There are indeed cases of Air Forces and other military services being shaped by such exercises; if the 1943–45 plans shaped the size or composition of the future size of the U.S.A.F., then they were indeed worth study even if no one in 1944 realized their significance. Yet the book does not settle this issue of impact.

A final question arises on what the substance generated by these committees demonstrated about bureaucrats in general, military or civilian. Smith broadly seems to acknowledge that the independence and relative status of the Air Force as bureaucratic goals explain many of the opinions endorsed on the post-war world. Yet the author avoids committing himself to any particular theory of bureaucratic politics and its interface with the rational public interest. His evidence is grist for such theorists' mills, but he does not etch out his own theoretical assumptions very clearly, nor does he cite many such theorists as they might apply.

The author suggests a budgetary imperialism model at points, in that the Air Force aspired to autonomy in the 1930's in hopes of getting one third of the military budget, but aspired to the same autonomy after 1945 in hopes of winning almost the entire budget. Yet it is not clear that the book is (or should be) asserting budgetary expansion as the unifying theme for all such bureaucratic rivalry. The mere word "autonomy" suggests other considerations besides size of budget, e.g. immunity from being overruled on its allocation.

All in all this is an interesting and well-written book. If only we could tell how important its subject was.—George H. Quester, *Cornell University*.

On Understanding Poverty: Perspectives from the Social Sciences. Ed. by Daniel P. Moynihan (New York: Basic Books, 1969. Pp. 425. $10.00.)
On Fighting Poverty: Perspectives from Experience. Ed. by James L. Sundquist (New York: Basic Books, 1969. Pp. 256. $8.50.)

These two collections of articles, both produced as part of a seminar on poverty at the Brookings Institution (Washington, D.C.), are an excellent representation of ideas currently held about attacking poverty. The two books manage to show the reader the best of these ideas and the inadequacies of these ideas. They are useful for presenting a significant controversy about the causes of poverty, for including several outstanding articles, and for provoking thought about the inadequacies of poverty research.

The significant controversy about the causes of poverty is described in a simplified manner by Daniel Moynihan:

> . . . opinions can be seen to cluster around two general positions—distinct, but not entirely incompatible . . . the question about the poor is whether they really are different or simply have less money. (vol. I, p. 23.)

If the poor 'really are different,' then an anti-poverty strategy would have to emphasize the improvement and expansion of social work, so that case workers could have more of an impact on poor people. The poor person would be 'educated' out of his lethargy and into the active, productive citizenship of a middle class person. On the other hand, if the poor 'simply have less money,' then an anti-poverty strategy would emphasize a guaranteed annual income. In general, sociologists argue for an expansion of social work, and economists argue for a guaranteed annual income. Moynihan claims that the failure of the war on poverty can partly be traced to intellectual disagreement on the nature and causes of poverty.

Moynihan describes the gravity of the disagreement:

> It was thus a matter of considerable interest and some unsettlement for the seminar to realize, after a preliminary *tour d'horizon,* that many persons who had been speaking the same language had nonetheless very different meanings in mind. There was no common understanding as to the nature of poverty or the process of deliberate social change. (vol. I, p. 19)

In this respect, the main failing of the book is the omission of transcripts of the conference sessions relevant to this disagreement. The dialogue is referred to in various articles, and James Sundquist partially describes it in "The End of the Experiment," but the reader is mostly forced to construct the dialogue in his own mind, by reading the various articles and specifying disagreements and possible resolution of disagreements. Readers are not likely to do this.

For this reviewer, the best resolution af Moynihan's controversy is found in Robert Levine's article, "Evaluating the War on Poverty." Levine speaks as an OEO (Office of Economic Opportunity) administrator, from the frame of reference of cost-benefit analysis:

> Let it be said at the outset that we define poverty in terms of command over economic resources and that this definition is not universally acceptable. . . . Let it be said in partial pacification of those who would define poverty in less economic terms that the economic definition goes back through the intricacies of poverty to end up with no less emphasis on noneconomic programs than would a noneconomic definition itself. (vol. II, p. 189)

Cost-benefit analysis tends to resolve conceptual differences between sociologists and economists into a search for agreement on the values to be achieved.

Several of the articles are outstanding. Daniel Moynihan's "The Professors and the Poor" is one of these. He gives an interesting chronology of a change in the public mind, from seeing the war on poverty as independent of race, to seeing that war as a 'black problem.' I refer above to his claim that the failure of the war on poverty can be traced to intellectual confusion about the causes of poverty. This condition is traced in turn to two conditions: (1) we lack reliable data about the effects on life styles of increases in income; and (2) we analyze poverty without the experience of being poor ourselves.

Otis Dudley Duncan's article, "Inheritance of Poverty or Inheritance of Race?", uses correlational and causal model techniques on national survey sample data, to show that race, independent of educational and family background, has a large effect on income differences. His article is the most methodologically sophisticated in the two books, and his comments on the policy implications of his findings are excellent. However, he uses bela weights from regression equations with very low (i.e. unreliable) correlations.

Oscar Lewis is brought in as the token anthropologist, but his article on "The Culture of Poverty," reprinted from his book, *The Study of Slum Culture,* is probably the best of the two books. He gives the reader an understanding of this culture as a coherent set of beliefs that rationally deal with slum situations, and that is likely to persist in spite of economic advances by poor people. Lewis' article goes a long way toward countering Moynihan's claim that a middle class intellectual must remain confused on the subject of poverty because he has never been poor himself. Lee Rainwater competently carries the reader further into a particular aspect of this culture in "The Problem of Lower-Class Culture and Poverty-War Strategy." He uses interview data from a black, public housing project in St. Louis, to show a 'tremendous disjunction' between sexual behavior and sexual norms, as well as a capacity to live with it:

> The great discovery that lower-class people make, which middle- and working-class people find so hard to understand, is that it is possible to live a life that departs very significantly from the way you think life ought to be lived without ceasing to exist, without feeling totally degraded, without giving up all self-esteem. (vol. I, p. 238)

Rainwater argues cogently that poor people cannot establish their own set of norms more in line with their own behavior: they are condemned to their culture of despair until society sees fit to establish a 'floor on family incomes' relative to the national, median income. Such a floor would eliminate the 'present situation in which some families are so far out of the society that the questions of motivation for achievement become really meaningless.'

"The Politics of Local Responsibility: Administration of the Community Action Program—1964—1966" by John Wofford, and "The View from the City: Community Action in Trenton" by Gregory Farrell are examples of that rare chronology which is theoretically interesting. Wofford gives a fascinating account of the development of disappointment and frustration relevant to two OEO goals: (1) reducing bureaucratic red tape in Washington, and (2) promoting consensus politics at the local level. Farrell describes a trial and error process, marked by eventual and perhaps exaggerated success, in building a local action organization; there are several insights, including some on handling clients and recruiting and paying personnel.

Finally, Peter Rossi has an interesting article on methodology, "Practice, Method, and Theory in Evaluating Social-Action Programs." He has a good discussion of the benefits and costs involved in choosing between experimental and ex post facto research designs. In a discussion of the politics of evaluating, he argues that administrators should be forced to commit themselves in advance to a reasonable course of action if research produces unexpected findings.

In sum, the two books contain several good articles that make up for the mediocre ones. However, they still leave the reader confused on the causes and cures of poverty, as everyone expects. Perhaps poverty research needs to be redirected toward a consideration of slums as part of an urban system with a supply of jobs and housing and a rate of arrival of migrants to the city, more in accord with the systems analysis used by Jay Forrester in *Urban Dynamics.* It is this reviewer's suspicion that many of the problems discussed in *Perspectives on Poverty* will only be understood and solved if urban systems analysis is used, even though it is ignored in the two volumes.—TERRY JONES, *Bowling Green State University.*

City and Country: Rural Responses to Urbanization in the 1920s. BY DON S. KIRSCHNER. (Westport, Conn.: Greenwood Publishing Corporation, 1970. Pp. 279. $11.50.)

Richard Hofstadter and other "revisionists" writing during the 1950's charged that some historians had glamorized rural America by emphasizing its decent leadership toward economic justice, meanwhile overlooking its sponsorship of irrational movements that burdened the American system, such as Prohibitionism in the 1920's and McCarthyism during the 1950's. The objective of these movements was to salvage rural America's declining prestige, and they took place during periods of rural prosperity, according to the revisionists.

Kirschner tested this thesis by taking a close look at rural attitudes and behavior in Iowa and Illinois during the 1920's. He concluded that the 1920's were a decade of economic hardship, not prosperity, during which farmers constantly sought economic remedies. But he agreed with the revisionists that Prohibition was a salient issue, uniting rural America in a fervent crusade against "the city." Prohibitionism and related cultural issues "lived a life of their own and flourished on their own terms," (p. 243) unaffected by the high rate of bankruptcies and the low price of corn.

Kirschner argued that Prohibition was not an "irrational" issue, if it is rational to seek status. He said that the 18th Amendment was ingenious in permitting rural America to enshrine its values, and in giving rural citizens reason to hope that these values, as part of the Supreme Law, would prevail. "Surely," he said, "there was no reason for them to suspect, before the fact, that the experiment would be such a shabby failure." (p. 243)

Kirschner said rural Americans really feared the immigrants who filled Chicago and flourished in its grime and noise. Henry A. Wallace, influential young editor and plant geneticist, said farmers realized the genetic consequences of sharing America with "people of low grade intelligence from southern and eastern Europe." (p. 37) Wallace proved statistically that cities sapped the procreative powers of "native" Americans, while not affecting the new races, and therefore cities could only become "the death chambers of civilization." With stakes so high, ruralites were right to fight back, but Kirschner's theory that Prohibitionism was mainly a "rational" outgrowth of a uniquely rural racism is somewhat confounded by the Al Smith campaign. Kirschner said ruralites abhored Smith because he personified the ambition of the immigrant, but in fact Wallace and other Midwest farm leaders supported Smith's candidacy (attracted by his farm plank), and Smith ran well in rural Illinois and Iowa, compared with previous Democratic presidential candidates.

The author was honest in noting other, less "rational" motives for Prohibitionism. The 18th Amendment enabled rural minds to draw a geographic line between good and evil—between the rural districts af Illinois whose representatives voted for the Amendment, and the urban districts which opposed it then and subsequently. To draw such a geographic line required a superb hypocrisp—overlooking rural carousing and crime, refusing to concede that rural hearts were already being won away from simple living by the materialistic appeals in the ads, movies, and on the radio. Kirschner said, "Unwilling at first to surrender to the larger meaning of that choice, they mounted an assault against the new urban America, as if the smoke and sounds of battle would hide their shame from the enemy, and more important, from themselves." (p. 252) Yet as the decade went on rural Americans became less anxious rather than more anxious to impose fundamentalist values on others. In 1926, almost half of the rural referendum voters in Illinois seemed ready to give way on Prohibition. (pp. 93–94)

For measures of rural attitudes and behavior, Kirschner relied mainly on editorial comment appearing in about three dozen small town nswspapers, and roll call votes in the two state legislatures. "Typically it was in the country press that the rural mind was reflected and reinforced," (p. 261) Kirschner asserted. Kirschner selected legislative roll call votes which reflected a rural-urban division. He separated these by subject matter and placed them in time sequence. Each vote was broken down by party and by rural-urban (with subdivisions for Chicago-other cities, and for rural poor—average—above-average areas). He provided his eyeball interpretation of each raw vote breakdown, aided by accounts of the debates. Most of the significant votes were entered as tables, but it was not clear what the reader was supposed to learn from them. Kirschner's interpretations were sensitive and usually persuasive.

On cultural issues, rural legislators usually maintained a united front, and urban legislators did not. On economic and governmental issues the reverse was true, permitting the cities to protect and enhance their economic interests. Rural legislators were pragmatic in their choice of economic and governmental objectives but lacked the self-confidence to bargain, when this was desirable. They preferred to try to checkmate the cities by refusing to reapportion seats, by refusing to authorize the use of the initiative and referendum, and home rule, and by keeping certain subjects outside the law. Rural legislators lost their most important objective—property tax relief—because they refused to allow cities to raise money by other means. The important new expenditure of the decade was for highways, and conflict among rural regions permitted urban legislators to negotiate the formulas for dividing road funds. Exemplary of rural intransigence and its results was an Illinois constitutional convention in which rural delegates had their way on virtually every issue, and thereby assured defeat of the proposed constitution at the hands of urban referendum voters.—DON HADWIGER, *Iowa State University.*

Public Housing: The Politics of Poverty. BY LEONARD FREEDMAN. (New York: Holt, Rinehart and Winston, Inc., 1969. Pp. 212. $3.95.)

A science of public policy captures the imagination of most social scientists. Currently the effort of making scientific political value judgements holds captive a great deal of scholarly time and libidinal energy, a great many pages of professional journals, periodicals and books. Ostensibly Edgar Litt's Public Policy Studies in American Government Series portrays applications of the analytical skills of the political scientist to the "output" side of public policy. His fourth choice of manuscripts, Leonard Freedman's *Public Housing,* contributes to the proliferation of pages of legal description and subjective evaluation of policy; however, the choice does little to extend scientific understanding of the relationship between public policy decisions and political behavior. Consequently, the book fails to illustrate the skills of the political scientist qua scientist.

According to Freedman, American government and the American political system work against the interests of the poor. Like any other book with a thesis, debatable alternative theories compete to explain its contents. Since the tests of the validity of a debate rest with the rhetorical and argumentative skills of the debators rather than with the rigors of scientific disproof, Freedman might have done better by advancing his implicit thesis of political behavior: to wit, propitious times and great men make public policy—ill writ or well writ for the poor.

This thesis better describes Freedman's presentation of fact and fancy in the evolution or devolution of public housing policies within the United States. Accordingly one learns upon reading the first chapter of the book that the Great Depression, an enlightened Democratic president, and a Democratic majority in the House of Representatives are the essential ingredients for the promulgation of political rewards to benefit the poor. The wartime housing shortage provided the circumstances for the support and expansion of public housing by the executive branch of government through to 1949. Times changed in 1949 with the election of the 80th Congress and Jesse Wolcott's ascendency to the chairmanship of the House Banking and Currency Committee. While the Democratic majority remained and while other "enlightened" leaders returned to their seats in Congress, they were not enough to protect enlightened public housing against the Republican minority and Wolcott's political cunning.

Of course when times and Congress turn against a given policy there always remains the hope of executive succor. Fate and the election of another Great Man—Eisenhower—dashed these hopes against the concrete abutments of private housing and freeway development. And so Freedman unfolds the acts and actors, the plots and sub-plots of Public Housing Policy for the reader.

Perhaps in the second millennium there will be another Great Depression, another Roosevelt, World War II, Wolcott, and Eisenhower and then the political scientist will have his chance at scientific test. Until then he will have to assuage these inclinations to tests of the more generalizable parts of the book. These are found in the chapters dealing with poverty, race, and public ownership.

In addition to the legislative and executive greats, pressure groups, poverty groups, racial minority groups and public attitudes toward public ownership also determine policy outcome. Freedman presents a cursory analysis of the role of pressure groups effecting the legislative process of legitimizing public housing. The analysis is descriptive and chronological in development. The chapter contains a rather exhaustive check list of formal organizations and their leaders. These organizations and leaders are classified as "opposition groups" and "pro-public housing groups." No empirical rationale is provided for this classification nor is any attempt made to analyze the extent or agreement or disagreement over means and ends between and among the two categories of pressure groups. The reader doesn't know whether the system of classification is mutually exclusive or overlapping. An analysis of membership overlap or pressure group interests could have solved this problem. Such an analysis would also have gone a long way in developing the science of public policy.

The intellectual influence of Oscar Lewis, Kenneth Clark, Michael Harrington, Harrison Salisbury, and Morton Deutsch reflects directly in the generalizable nature of the analysis of poverty and racial influence. If public housing is a result of grass-roots movement then Oscar Lewis' findings on the cyclical nature of poverty and the notorious social and political disorganization of the poor suggests the demand is not theirs. Clark's, Harrington's, and Salisbury's independent observations further suggest public housing to be a middle-class defense against the poor. Since public policy must be consistent with the dominant ideology or ideologies and since the American ideologies are hostile to concentrations of public power and public ownership, government housing for and by the poor is virtually impossible. Site restrictions, administrative restriction, and cost-design restrictions also militate against an interpretation of public housing as a product of altruistic concern for the plight of the poor and racial minority groups. Freedman finds racism in realty practices and the threat of racial insurgency partially explain the entry of Black people into public housing and the exodus of poor whites steeped in the racism of the dominant classes. He overlooks or leaves for inference the implications of public housing on the "block busting" practices of realtors. These implications may explain some of the upper-class resistance to governmental redistribution of housing values.

An evaluation of the book and the research it represents raises one of the larger questions before the discipline of Political Science. Have we been too rigorous and dispassionate and do we now need descriptive studies relevant to policy makers? This book is intended to answer the latter need. It partially succeeds by identifying private and public leaders, their organizations, their laws. It fails by ignoring the need for analytical frameworks to guide future quanitative analysis of actors, their strategies and their policy outcomes. The meaning of many of Freedman's facts remain uncertain. The book is, perhaps, a natural and necessary precursor to more scientific investigations of the same phenomena. In the development of scientific knowledge, the subjective and normative presentation of facts and values appear first on the scene. Later the more rigorous student subjects some of the assumptions and inferences to tests of disproof.—R. E. JOHNSTON, *Wayne State University.*

Campaigns: Cases in Political Conflict. BY WALT ANDERSON. (Pacific Palisades: Goodyear Publishing Company, 1970. Pp. 242. $6.95.)

Case studies of significant national and state political campaigns are excellent material for understanding as well as for teaching when authors do their homework on the superabundance of available information and without naivete about the realities of political life. But this volume fails to relate in depth the campaigns to yesterday or today.

The good guys ride white horses in stereotyped silhouettes. The author imagines that many candidates smeared as Socialists by opponents are really such and is sure that only "radical theorists" think labor and farmer are allies. Presidential primaries did not diminish because of the disillusionment of idealists but because of their inconvenience to political machines. The reader does not discover the calculated ambivalence of Lincoln's 1858 statements on race nor that a change of 1 in 1,000 votes would have cost him both presidential elections. Bryan and La Follette are replaced by Theodore Roosevelt as the Progressive leader and Pershing's black cavalry at San Juan Hill in front of TR is only one of the missing ingredients of the TR story. La Follette is accused of being a dictator, though TR is not, and is slandered with the resurrected legend that the Wisconsin Progressive was unbalanced because he criticized newspaper publishers to their faces. After LBJ, can TR be "the most energetic president"? Needlessly superficial are the Kennedy and Nixon chapters and to conclude that the religious issue was buried in 1960 is to misread a close election and to forget that New York has had no Catholic governor since 1928. Also omitted is the understanding that the 1960 McCarthy campaign in New Hampshire was a facsimile of the 1952 Kefauver race, triggered by a blunder of the same presidential assistant and dependent on Republican finance as well as publicity.

Skimmed over the surface also are state campaigns, with serious omissions and errors. Wade Hampton was not chief of Confederate cavalry nor Benjamin Tillman a possible presidential nominee, though his sparking primaries over the nation is almost ignored. The "composite" picture of Senator Millard Tydings, Sr., and Communist Earl Browder is not clearly identified as a fraud; and also unstressed by the author who makes the San Fernando Valley his home is the management of the smear campaign against Upton Sinclair by Earl Warren. Pumpkin papers are swallowed as uncritically as the La Guardia legend. Is it correct that "Reagan's chief interest in college was the student body"? Callaway Gardens is not in Columbus, Ga.; and Carter did not lack for outside finance. And missing from the account of the Lester Maddox and "Bo" Callaway campaign are many stories of interest, such as Lester's "Do you want a man for governor who never had to look for a job, never had to read the want ads, never had to wait for a pay check, never had to buy on the installment plan, never had to put off buying anything?", along with a former governor's pleading "There is a lot of difference between a man who knows better and one who doesn't," and the letter in the Atlanta *Constitution* "To Avoid BO, use Lesterine."

When Alfred Tennyson wrote of "the fairy tales of science," he did not envisage political science.—CHARLES G. HAMILTON, *Aberdeen, Miss.*

COMPARATIVE GOVERNMENT AND POLITICS

L'Organizzazione Partitica del PCI e della DC. BY F. CERVELLATI CANTELLI, V. CIONI POLACCHINI, P. DE VITO PISCICELLI, S. GUARINO CAPPELLO, G. POGGI, G. SANI, G. SIVINI, A. SIVINI CAVAZZANI, a cura di Gianfranco Poggi. Istituto "Carlo Cattaneo," Vol. II, Ricerche sulla Partecipazione Politica in Italia (Bologna: il Mulino, 1968. Pp. 591.)

No one interested seriously in Italian party politics should go very long without reading this book, the second volume in a series focussed on political parties and participation produced by the noted Carlo Cattaneo Institute of Bologna. A while ago, an Italian political scientist complained that empirical studies of Italian politics seemed to come mainly from foreigners. The authors who contributed to the three sections of this new volume have done much to overcome this imbalance. Using varied, and, in the main, effective tools of analysis, they approach the internal life of the two major Italian parties—the Communists and Christian Democrats—from three different standpoints: organization, mass membership and leadership. They come up with a product that is part exposition of research findings and insights, and part compendium of basic background data for the study of political parties in Italy.

The section devoted to the organizational life of the two parties, prepared by Giacomo Sani and Stefania Guarino Cappello, demonstrates how a thorough analysis of the party literature and documentation, carried out by a scholar with a real sense for complex organizations, can make an important contribution to an understanding of party behavior. (This is more true of the Communists, who have produced a body of documentation immensely richer and more descriptive of their organization than have the Christian Democrats). The chapter by Sani on the PCI as an organizational system shows both the genius for synthesis that is characteristic of Italian scholarship at its best and a thorough empirical grounding, something often lacking in work by Italian scholars. The synthetic chapter on the DC is, in contrast, disappointing, as is the absence in this hefty essay of any real framework of comparison. For instance, much of the evidence collected on the PCI suggests that its organizational problems are universal (and not uniquely Leninist) and result from its leaders' self-conscious decision to work within the political system as legitimate participants. Some of the same problems seem to afflict the DC, but in the absence of an explicit comparative framework, it is hard to know exactly how similar the difficulties of the two parties are.

Explicit comparison is one of the major virtues of the section on the two parties' mass membership, a statistical and historical analysis by Giordano Sivini and Ada Cavazzani. (A fourth aspect —the activists—appears in a separate volume, *L'Attivistà di Partito,* edited by Francesco Alberoni). In this section, some standard aspects of organizational sociology—the number, composition and consistency of the registered members, the relationship of members to voters, the rather special problems of organizing among the young and the female—are treated with both the historical flair that is traditional in Italian scholarship and with a statistical finesse that will serve many a future scholar with a precious resource. With all that, the section has less of a theoretical structure than the first, and one somehow feels that more might have been done with the material in the way of inference and interpretation. Sivini does do more in a later article, and there is much promise of more to come from his direction in the study of political parties.

The third section of the book, contributed by Gianfranco Poggi and Vittoria Cioni Polacchini, examines the whole topography of the "classe dirigente" of the parties. The Italians have contributed this term to the world. They are never without skill in probing the phenomenon, as Poggi shows in his theoretical introduction. But the treatment of the statistical data, drawn from questionnaires collected from parliamentarians and organizational leaders of both parties, adds little to the standard presentation of background, recruitment and career pattern variables that have become a standard implement of political sociology. Moreover, and here we anticipate a general criticism of the volume, the research on the leadership of the two parties is not at all related to the two previous sections of the book, with the result that the overall impression is of three strata of analysis, all obviously interdependent, but presented in airtight packages.

There are several reasons for this general defect. First, as Poggi tells us in the general introduction, in the planning of the larger project, the Cattaneo Institute allocated "a maximum of autonomy" to the participants. But, second, the problem is compounded by the fact that the work in this volume was divided into concrete, rather than analytical, slices. Thus organizational history from 1946 to 1963 is treated in two different guises, but the *process* of decision-making within the organization falls between everybody's stools. This most critical aspect of organization ought really to have integrated the volume, and in its absence, the overall impression of the volume is more of a source book on Italian party organization than of a report on five years of Cattaneo Institute research.

A second basic problem, already alluded to above, is the lack of an overall conceptual framework to inform the work, not only of these authors, but of the Cattaneo participation series as a whole. One must be satisfied with a rather broad inventory of "meanings" of the term "participation" in the general introduction, but the term has no fundamental role in the general strategy of this series of volumes. Paradoxically, then, *L'Organizzazione Partitica del PCI e della DC* is weak where Italian scholarship has generally been strong—in general theory and in the elaboration of a conceptual apparatus—while it excels in the solid empirical grounding that foreigners have sometimes found weak in Italian political writings. Whatever the cause, the fault does not lie with the authors of this volume, who have carried out the tasks assigned to them supremely well.—SIDNEY TARROW, *Yale University.*

Le vestali della classe media: Ricerca sociologica sugli insegnanti. By MARZIO BARBAGLI AND MARCELLO DEI. (Bologna: il Mulino, 1969. Pp. 373. L.4.000.)

Most American political scientists are unfamilar with the degree of theoretical and methodological sophistication now extant among Italian social scientists. This impression has persisted despite laudable efforts to correct it by such scholars as Joseph LaPalombara, Sidney Tarrow and Samuel Barnes. Several recent volumes published by *il Mulino,* one of which is reviewed here, should do much to demonstrate that the work of Italian scholars can be neglected only at considerable intellectual cost. In this connection one should note the following titles; Giorgio Galli (ed.), *Il comportamento elettorale in Italia,* Gianfranco Poggi (ed.), *L'organiz-*

zazione partitica del PCI e della DC; Francesco Alberone (ed.), *L'attivista di partito* and Agopik Manoukian (ed.), *La presenze sociale del PCI a della DC.*

Le vestali della classe media by Marzio Barbagli and Marcello Dei is one of the volumes which should not escape notice. The authors are generally concerned with investigating the relationship between public policy, in this case educational reform, and the Italian system of social stratification. In particular they focus on the impediments to governmental reforms aimed at enlarging the social composition of the scuola media. They suggest that the primary source of resistance originates with the teachers and principals who fundamentally oppose reform and are openly hostile to students from the lower classes. Moreover they observe that the teachers reinforce the prevailing social structure by rewarding values of passivity and obedience rather than independence and creativity.

In order to test these insights, these scholars construct a complex research design through which to investigate the phenomenon in question; namely resistance to change in the school. They ground their inquiry in a large body of survey data. These data consist of lengthy interviews with 374 teachers, 200 parents of students and 328 mail questionnaires completed by school principals. The interviews were conducted between 1966 and 1968 in Florence, Bologna, Pistoia and Arezzo.

The volume's four chapters draw heavily on these interviews yet each reveals a neat mesh between theory and data. Chapter one focuses on the sociological configuration of the teaching profession in order to bring into sharp relief the social structure of the new scuola media. The authors present a number of interesting findings concerning entrance into the profession, professional satisfaction, and the relationship between perceptions of professional prestige and status insecurity. Chapter two employs a multi-variate approach to explain the central phenomenon of resistance to change in the schools. Chapter three concerns the attitudes of teachers towards sex education as predictors of their general resistance to change and as an indicator of their unwillingness to engage in genuine intellectual exchanges with the students. Chapter four brings the whole volume together by examining the changing role of the teacher in Italian society and the attitudes of parents and principals toward the role. The chapter also contains important evidence on the propensity of teachers to socialize the students to subordinate roles. Finally, there is a methodological appendix and a copy of the instruments used in the interviews.

The findings of chapters two and four are of most interest to the political scientist. The first rigorously isolates the causes of resistance to change in the schools via the construction of four Guttman scales which are entitled as follows: 1) Resistance to Change; 2) Personal Rigidity; 3) Authoritarian Teacher; 4) Political Conservatism. Age and the profession of the father are also considered. The Simon-Blalock causal modeling technique is used to sort out the most acceptable models for male and female teachers. Among the men the most powerful predictors of resistance to change are political conservatism and social background, while among the women the most important factor is political conservatism in combination with age. The fact that resistance to change is rooted in political conservatism is indeed a significant finding and should serve to stimulate further research along these lines.

In chapter four, Barbagli and Dei make a largely convincing case for their proposition that most of the teachers in the scuola media use the sanctions of their profession to control political and social deviants. Their case would have been even stronger, however, if they had gathered data on *student* perceptions of the restrictive nature of the teachers behavior, instead of making inferences about "socialization to subordination" on the basis of data from parents, teachers and administrators.

In sum, the volume is an extremely important contribution to our understanding of the attitudinal and societal features of the Italian teachers' role which tend to reinforce mechanisms of societal exclusion and impede public policy intended to democratize the Italian political system.—Timothy M. Hennessey, *Michigan State University.*

Politics and Change in Developing Countries: Studies in the Theory and Practice of Development. Edited by Colin Leys. (New York: Cambridge University Press, 1969. Pp. 289. $7.50.)

A collection of nine papers from a 1968 conference at the University of Sussex, Institute of Development Studies with introduction by the editor, now of University College, Nairobi. All are about development; all are by Britons; all are eminently scholarly. Some are genuinely interesting to developmentists, either because they put part of the field into excellent perspective or because they suggest interesting approaches or avenues of research. A large bibliographical index is useful.

Leys, who has encouraged many young students of development, sympathizes with those who have suffered ". . . the outpouring in recent years of collections of papers in the social sciences"—the ". . . things in books' clothing. . . ." Defensively, he argues that this collection is more—that, in it, is achieved a synthesis of English traditions of historical empiricism and political theory with American behavioralism, seen as Parsonian sociology carried to excess.

The book's contents do tell us something about movement in British political science, but they do not demonstrate the emergence of a neo-classical synthesis or rebirth of theory. The thread Leys finds to link them into a book is weak; the essays must be judged separately.

Robert Dowse's essay on the *military* and development is an excellent, scholarly account of what we know, what we don't and why, and what we should do now. The field is cramped by militaries' dislike of being researched and by the vagueness of our categories. The latter has affected that part of the field academics have concentrated on—coups. We have largely neglected how the military acts once in power, its political influence when not in control, and even the social background of its members. The author reviews the results of factor analysis—an effective Occam's Razor even with sparse and dubious data, but does not provide the needed "good theory at the right level of generality." An able discussion of the Ghanaian military illustrates some of his ideas.

Morris-Jones neatly situates the concept of *political recruitment,* as yet little used in less-developed countries, and reviews its utility—both sufficiently general and sufficiently precise (more so than mobilization, for instance). He notes that the term implies the selecting agency's exclusive initiative, which is hardly the case. Much can be learned about development, he suggests, by widening the study of recruitment (entry?) through work on 1) typical career routes (via biographies), 2) aspiration patterns (which sorts of people are clamoring to be recruited and which are not, and 3) catchment areas (a category less clearly explained).

When Morris-Jones studies recruitment of members of the Indian parliament and state legislative assemblies, one has to agree that use of the concept to elucidate development politics is just beginning.

Feldman examines the consistency of *Tanzanian agricultural modernization* with the government's cooperative *ujamaa* ideal. His empirical base is narrow: his wife's work with 49 tobacco farmers and P. H. Gulliver's on Nyakusa land tenure. Nevertheless, the generalizations are convincing (and devastating for the *ujamaa* ideal) and the conceptual framework, full of promise for future research on rural change.

Instead of asking whether local society is resistant to social disturbance, Feldman looks at the *economic stress* imposed on traditional ways by new development options, i.e. the cost of not changing. That cost measures loyalty to traditional patterns and explains selective adoption of innovation.

In commercial agriculture, the economic stress of cooperative management is found to be high; *ujamaa* organization declines with modernization. Social stratification has not yet appeared, because land is a free good; hired laborers are not proletarized, but labor by choice and have access to land. Growth of population and commercial agriculture, however, will create land scarcities and stratification.

Alec Nove's lucid article on *Soviet development* and political organization contains little that he hasn't said before. It is interesting nonetheless, and refreshingly free from footnotes. Nove traces the victory of "teleological" over "genetic" planning, culminating in Stalinist contralized growth policy. He is careful to count the economic costs of terror, of the primacy of politics, and of a pricing system in which planners could not calculate alternative costs. He suggests how the results might have been bought more cheaply, but recognizes that Russians were plowing a new furrow and that it is illusory to believe that high Russian rates of accumulation could have been achieved without severe repression.

The article is on the margin of this collection. Nove admits that 1918 Russia was not the low-income countries of today, saying only that all countries are unique and Russia's experience is one source of lessons. Other articles, like Feldman's and Vincent's, emphasize the limited administrative capabilities of today's less-developed countries and their distance from Soviet experience.

Colin Leys points to the lamentable consequences of letting economists do all the writing about *planning.* What planners actually do is not—probably could not be—the "synoptic" process Tinbergen describes. Business-firm management furnishes the closest analogy to planning; Leys looks there for analytic clues.

The most useful distinction—between plan-making and implementation is hardly revolutionary. Most important is the urging to governmentists to study planning as politics, plus the methodological advice: look for 1) type and distribution of authority in the planning system, 2) "activators" available, and 3) mechanisms for decision-making. Leys' discussion of Tanzanian planning is instructive.

Despite its heavy scholarship and footnoting, Joan Vincent's article on *anthropology* and development makes a number of telling points about the peculiar strengths of her discipline. Political scientists recognize that at least the newer low-income states tend to be, as Zolberg says of Africa, ". . . an almost institutionless arena. . . ." It is the pre-state institutions that count, and that is what anthropologists study. They generally work in the field; governmentists in the capitals. The former are running out of closed societies of primitive people to study, so they are ready to take on development. One may not agree, for instance, that stratification is the essence of the modern state, but it seems obvious enough that good political science of development should be much more anthropological.

Peter Nettl, who died tragically in 1968 and to whom the collection is dedicated, contributed a very literate article on *development theory*. The universal priority of development is asserted; the author emphasized that we don't agree completely on what it is and that existing theories are better at pointing up problems than at providing solutions. Theory is essential; without it we are doomed to gather data at random. However, the article concludes with the need unfilled. As a starter, the author suggests loosening the connection between economic development and other types, and giving up teleological notions that equate development with Westernization and minimize the contribution of the poor to the rich. Attractive as these notions are, this reviewer feels that following them sacrifices what little conceptual clarity we have and misunderstands much of the drive for development.

Surveying *single-party politics* in the *Ivory Coast*, Martin Stanisland argues that the party's successful takeover of the bureaucracy had led to its bureaucratization. His beginning is certainly promising; he castigates existing literature on African politics as banal and circular as a result of 1) ingenuously believing what ruling elites say about themselves and 2) using sloppy definitions and concepts. The reader's appetite is whetted but, unfortunately, he is in for a disappointment.

Staniland's analytical concepts—"levels" and "arenas" at/in which "political resources" are used by politicians to capture "prizes"—do not appear either very novel or promising. His recapitulation of Ivorian party history, drawn from well-known sources, is marred by slightly reckless generalizations and annoying factual errors (Ivory Coast is called a French overseas department—p. 144; most French post-war investment is said to have been drawn from the local budget—p. 160; . . .). Staniland destroys a few straw men, then elucidates the state of the party as revealed by his 1967 research in three administrative towns. The conceptual framework reveals nothing that would have been hidden from the informed layman.

Shafer's prolix article on *public administration* leaves developments in this field exactly where the author began—deadlocked. He complicates his task by trying to account for everything administrators do, whether administration or politics.

The "deadlock" is the realization that Western public-administration precepts contain cultural bias and that applying them uncritically to development situations, as "scientific" managementists once advocated, might not be appropriate to rapid change. Bureaucratic administration fits this pattern too, Shafer says, but he fails to resolve on an alternative. Politicization of administration may be good, or it may not.

The author's only suggestion to planners is to try taking administration advice from temporarily posted, ad-hoc teams. Those experienced with short-term, visiting experts in low-income countries (mea culpa) will cringe at the thought.—W. I. JONES, *Oberlin College, Oberlin, Ohio.*

Abgeordnete in der Parteiendemokratie. BY PETER GERLICH AND HELMUT KRAMER. (Vienna: Verlag fuer Geschichte und Politik, 1969. Pp. 246. öS210.-)

This study of the political subculture of the *Gemeinderat* of Vienna, which is at once municipal council and state diet, leans heavily on the approaches to the study of legislative behavior introduced by the writings of John C. Wahlke and Heinz Eulau. The latter, in fact, presided over the beginnings of the project in 1964/1965. Ninety-eight of the 100 members of the *Gemeinderat* were interviewed and some of their comments are quoted at length. The project sought to ascertain (a) the composition of the body, (b) the careers of its members, and their attitudes toward (c) their party and (d) their constituency, and (e) their conception of their functions. The authors claim only to have explored but not exhausted their stuly of the role conceptions and images of their legislators for lack of comparable exploration of the role conceptions of administrators, party leaders, journalists, associated representatives and other roles interacting with those of the members of the *Gemeinderat*. The quantitative manipulations in this study use two kinds of data: attitude sets, such as the segments of the legislators' role conceptions toward various other agents, and social background data taken mostly from official sources. The sociological information includes age, sex, previous occupation, outside occupations, seniority, rank or offices held, committee memberships and data on the members' political socialization.

Since the legislative setting in Vienna and elsewhere in Austria has not been sufficiently examined before, the authors set their empirical explorations into an ample framework of institutional description and historical sociology. The changing character of Viennese legislative assemblies and their composition over the last one hundred years gives meaning to recent changes in recruitment patterns. The evolution of the dominant political parties—the smaller ones are rather neglected in this book—explains some of the notable differences which emerge in their role conceptions and in the social backgrounds of their members. To the uninitiated mind, this historical and institutional background is quite indispensable to an appreciation of the empirical findings. In fact, the indispensability demonstrates the irrelevance of empirical and behavioral research when it is divorced from a thorough knowledge of the setting. The thorough coverage, in any event, should facilitate comparison with legislative behaviors and subcultures elsewhere, or at least protect the comparative researcher from the most obvious pitfalls.

Gerlich and Kramer proceed from the historical background to the sociological tabulations on the membership. Their findings that the *Gemeinderat* is more representative of contemporary Viennese society than were earlier legislative assemblies is contrasted with American findings and with the inherent contradictions of representative theory and practice. How can legislators be perfect "social representatives" of a society whose members have to work full-time at their trade and who have, precisely for that reason, delegated their representation to more or less full-time representatives?

The most interesting chapter of this study, to the mind of this reviewer, is the chapter on the political socialization of the deputies. Alone the incisive impact of historical events between the wars and the discussion of how early the respondents became involved in politics permit fascinating glimpses of Austrian political culture. There are, for example, significant differences between the Socialists (SPÖ) who start with politics early in life and under more family influence than members of the People's Party (ÖVP) who were often impelled more by friends, colleagues and associations. Youth organizations played an important recruitment function in both parties. The nomination patterns to candidacy for the *Gemeinderat* again differ substantially with the SPÖ deputies being placed in nomination chiefly by their district organizations while the party leadership and the chamber organizations play a larger role with the ÖVP. The authors also devoted some attention to the process by which new deputies are broken in, and to the place of *Gemeinderat* membership in the total career expectations of the deputies.

The rest of the book is a competent replication of the Wahlke-Eulau approach to the study of subjective legislative role images. Gerlich and Kramer segment the total role images in orientations vis-a-vis the respective party and the constituency from which the deputies were elected. Despite their emphasis on the stronger party role in Austria as compared to the United States, their treatment of this segment appears to be rather meager. The relations to the constituency, to the voters, special interests and associations, and the delicate balance between orientations according priority to the city over the districts and vice versa, on the other hand, are covered convincingly and in depth. The tabulation of role images of the decision-making postures—ritualists, popular tribunes, etc.—and the attitudes toward the administration and the clientele of voters and organized interests brings no great surprises. One would hope that the various replications of the *Legislative System* model might soon be subjected to systematic cross-national comparison.

The authors made no attempt to corroborate the subjective role conceptions of their legislators with their actual behavior. Thus, we are still groping in the dark regarding the relevance of this aspect of the political subculture of the *Gemeinderat* to its actual functioning. They did a considerable service, nevertheless, to the study of continental European legislatures by relating their concepts and findings to the ongoing debate about the decline of parliamentary democracy. Since this debate so often languishes in sterile juxtapositions of 19th century representative theories with mid-twentieth century political realities, the exploration of the subjective role conceptions of legislators in itself is likely to contribute to a broader appreciation of contemporary representative functions beyond myth and cultural pessimism. There can be little question but that the authors succeeded admirably in explaining some of the functions of the Viennese *Gemeinderat* to whomever cares to read their study.—PETER H. MERKL, *University of California, Santa Barbara.*

Le phénomène gaulliste. BY JEAN CHARLOT. (Paris: *Librairie Arthème Fayard,* 1970. Pp. 206. 24 F.)

Recherche sur le vocabulaire du général de Gaulle: Analyse statistique des allocutions radiodiffusées 1958–1966. BY JEAN-MARIE COTTERET AND RENÉ MOREAU. (Paris: *Armand Colin, Fondation nationale des sciences politiques. Travaux et recherches de science politique,* 1969. Pp. 247. 39 F.)

Le phénomène gaulliste raises high hopes, promising to "immerse the men of politics and a good number of the French in perplexity,"; nevertheless, the book is likely to prove disappointing to those familiar with Charlot's earlier effort, *L'U.N.R. Étude du pouvoir au sein d'un parti politique.* In his latest work the merchandise is somewhat worn and noticeably absent is reference to or development of phenomena that one might hope to find—e.g., relationships between Gaullism and the military, the full meaning of various important conflicts within the Gaullist organization, interaction in and after 1967 between Pompidou and Giscard d'Estaing, Gaullism and the handling of communications media (especially the *O.R.T.F.*). These are but some of the many *lacunae* characteristic of the study. Finally, the reviewer cannot help note in Charlot's study how *Le phénomène gaulliste* and *Le phénomène gaulliste imaginaire* occasionally get in each other's way, particularly when he claims that the *U.D.R.'s* internal convergences completely overshadow those contradictions harbored by it—and when he states that these contradictions most often contribute to the advantage of a modern organization engaged in the business of contributing consciously to the construction of a modern party system.

Charlot views *Le phénomène gaulliste* as a mutation of the French political system. The departure of de Gaulle marks for him not the end of Gaullism but simply the termination of "a Gaullism." Charlot finds—"in contrast to the findings of so many others"—that *Le phénomène gaulliste* is not simply a moment in an endless cycle but a truly different and durable development in French political life representative of the passage from a system of weak and multiple political parties to a system of the dominant party (perhaps even to bipartism?) and the birth of a "party of voters" based on other than cadres and militants. Moreover, this party's multiple ideological tendencies lack for Charlot the diversity characteristic of a "party of militants," contributing not to the *U.D.R.'s* disadvantage but to its "richness."

Charlot concludes that the opposition to Gaullism failed to fully appreciate its nature, taking cognizance of its leader while underestimating the support given the *U.N.R.*, *U.D.V.* and *U.D.R.* Believing that the organization would disappear with the departure of de Gaulle, the opposition ignored the constant reinforcement of another Gaullism—the Gaullism of a party—whose growth contributed eventually to both a qualitative and structural transformation of the party system. Charlot notes after 1958 that the history of Gaullism was one of ascending "new Gaullists" and descending "old Gaullists"; with the "arrival" in 1967 of the *modernes,* the organization underwent profound transformation—restructuring, liberalizing and modernizing itself. Finally, Charlot finds it "astonishing" that de Gaulle made foreign policy his *domaine réservé,* commenting that Pompidou's *domaine réservé* would be education, not national defense, and that after all these years of imperial grandeur Pompidou represents "perhaps" the "firm wiseness of a Louis XVIII."

Recherches sur le vocabulaire du général de Gaulle was reviewed extensively in France in 1969 and discussed on the radio-television service presentation *France-Culture.* French assessments of the work include, "a remarkable application of the *ordinateur* in the field of letters," "scientific and dry," not intended for *"esprits légers,"* etc., etc.

Cotteret and Moreau, who describe their book as at the crossroads of "four new sciences—political science, statistics, linguistics and communications," seek through use of the *ordinateur* to initiate a step in the direction of producing an instrument of study that can be applied to a vocabulary. Examining statistically the 62,471 words articulated in radio-television addresses by General de Gaulle between 1958 and 1965, the authors establish a typology that distinguishes between two kinds of speeches—*discours-appels* and *discours-bilans* (in the former utilization of the pronouns *je-vous* is elevated and use of the pronoun *nous* practically extinct, whereas in the latter utilization of the pronoun *nous* is elevated and use of the pronouns *je-vous* disappears almost completely). The authors determine if the speeches containing the pronouns *je-vous* have in common a series of words whose frequency of utilization is superior to the average utilization of the other words; the method then is repeated with the speeches containing the pronoun *nous.* Consequently, the authors discover around *discours-appels* a word-network consisting of *"république, état, peuple, moi, confiance, nation,"* and around *discours-bilans " notre, année, économique, monde, développement, progrès."*

Having established two types of *discours,* Cotteret and Moreau make the following comments on each. *Discours-appels* are brief, direct (so as not to weary the listener), rich in vocabulary and of a marked interpellative character, whereas *discours-bilans* do not interpellate, they merely state. *Discours-appels* seek to convince the listener that he is directly concerned, while *discours-bilans* seek to convey to the listener the feeling of having collaborated in what the General is summarizing. Among the *discours,* only three fall outside the authors' typology and all are related to the presidential campaign of 1965 (here the authors acknowledge that the usual schema is abandoned and replaced by a scene with but three actors—de Gaulle, the press corps and the French—and the General plays no longer with the public but before it, seeking to raise emotions and to give information).

The authors relate to the nature of the times and to de Gaulle's style and personality the frequency of his speeches, the number of words articulated by him, the length of his sentences and the richness of his vocabulary. Speeches delivered by de Gaulle at the beginning of his *septennat* were frequent and incisive; nevertheless, with the passage of time, his speeches became more "ample" and less percussive, with assurances to the listener that *tout va bien.* Although the length of de Gaulle's speeches generally did not vary, the length of his sentences increased with the years—convincing the authors that this acceleration was tied to the deintensification of the political events then in play (and leading them to suggest that "Perhaps the day will come when we will be able to measure the well-being of the state by the length of sentences of its political men?"). Finally, the authors conclude that the richness of de Gaulle's speeches corresponds very well to those texts written in modern French.

Recherches sur le vocabulaire du général de Gaulle searches for links between words and times and suggests the need for systematic analyses of the means of communication so as to facilitate new approaches to the study of political phenomena. Many of the techniques applied to de Gaulle's vocabulary will be familiar to those acquainted with efforts undertaken elsewhere by those involved in the study of linguistics. Finally, if the results obtained by Cotteret and Moreau are somewhat disproportionate to their prodigious efforts, this is entirely understandable in a work of this kind. *Recherches sur le vocabulaire du général de Gaulle* merits more than passing attention and one hopes that it will not be brushed aside in cursory fashion by those impatient with the methods employed in it.—Lowell G. Noonan, *San Fernando Valley State College.*

The Jana Sangh: Biography of an Indian Political Party. By Craig Baxter (Philadelphia: University of Pennsylvania Press, 1969. Pp. 352. $12.50.)

Until the Congress split in the fall of 1969 the Jana Sangh was India's second largest political party. It is the fastest growing party in India, and the only one of the major parties which is run and operated by persons with little or no background in the umbrella-like Congress Party. In this sense "illegitimate" in its birth, the legitimacy of the Jana Sangh is also doubted by many Indians because it challenges the fragile framework of secularism within which India's multitudinous communities live together. The epithets which it evokes from opponents—"right," "rightist," "reactionary," "communal" and even "fascist"—are an indication also of its organizational discipline and the militancy of the youth organization, the R.S.S., from which many of its members are drawn. Yet this large and controversial party has received little scholarly attention, and Baxter's study is to be welcomed.

Baxter frames his study as a biography, beginning with the party's origins in the Hindu Mahasabha, which arose in response to the Muslim League; turning then to its ideological and organizational ancestor, the revivalist Hindu R.S.S., which continues to supply the party organization with its very powerful secretaries. The major portion of the book is devoted to documentation of party growth and policy articulation, in which party conferences and Indian general elections are the major events. There is one chapter on each of the four elections which reports on party organization, manifesto, tactics, and results, ending with an appraisal of policy toward alliances with other parties. Alternating with these chapters are accounts of success in other elections, organizational development, parliamentary impact, and extra-parliamentary activities, which include the tragic Kashmir Satyagraha in 1952 that resulted in the death of the party's founder and the anti-cow slaughter agitation in 1966. The care in tracing changes in policy toward Kashmir, the national language controversy, foreign policy, and land reform is a significant contribution of the book.

Two themes of much interest emerge: the failure to effect electoral alliances, and the internal party conflict between R.S.S. and non-R.S.S. wings. The two are, of course, related. In the first two elections the party's most natural allies were the Hindu right parties which it unsuccessfully courted, losing seats which might have been gained if votes had not been divided. In the fourth election its most likely ally was the classically liberal Swatantra Party, with which a useful alliance was formed in states where the Jana Sangh was weak, but not in the two states where such an alliance would have enabled it to gain many more seats. On the whole, this is not a good record. Yet the arithmetic manner in which Baxter assesses the costs of non-alignments—adding the votes of the two would-be allies to determine if the total would have won the seat—provides only a partial assessment. More insight is needed into the reasons for the failure to align, which would enable an assessment of the costs of alignment as well, in terms of the seats the party would have forfeited to its allies, and compromises in the party program.

The non-R.S.S. wing of the party has been more willing to make these compromises than the R.S.S. dominated party organization. More attuned to electoral prospects, the non-R.S.S. leaders espouse the more pragmatic politics of negotiation on Kashmir, electoral alliances, and participation in coalition governments. The R.S.S. cadre are more concerned with cultural purity and cohesion of Hindu India, with an ambivalence toward politics which has made political investigation of the Jana Sangh difficult. They have urged the party to enter elections alone with the militant Hindu views for which the party is known—opposition to cow slaughter, recovery of portions of Kashmir now in Pakistan, etc. This is a familiar and important conflict in strongly issue-oriented cadre parties operating in electoral systems. Yet with little material on the internal party divisions in which this conflict is articulated, Baxter misses also its implications for the party's capabilities in the pluralistic social environment. Unlike the Congress, the Jana Sangh is not an aggregative party. Baxter finds an increasing flexibility after 1967 in adapting to local issues and power constellations, to the point of state units taking opposing sides on the same issue, but concludes that major concessions are unlikely. Though not explicitly linked to internal party affairs, this conclusion seems to indicate that the organizational cadre will continue their predominance. According to most analyses of Indian politics their non-aggregative model will limit the party's capabilities; yet so far the Jana Sangh has done remarkably well.

With such an emphasis on electoral politics, one misses much of the dynamic of this party. It is known for discipline in containing these factional conflicts, which Baxter attributes to the R.S.S. training of party cadre. Yet with no description of the early morning exercise sessions and the other modes in which the R.S.S. firms its recruits, the loyalty and militancy of the Jana Sangh cadre remain effectively unexplained. Similarly with no depth analysis of any of the communal incidents for which the Jana Sangh is frequently blamed, the party's relationship to a crucial set of emotional issues remains unarticulated. It is a mark of the scholarly tone of his work that Baxter does not place much emphasis or find a label for the party. Without materials of this kind, however, his characterization of the Jana Sangh as a conservative party appears more as caution than analysis.

There are more conceptual questions as well which one would like a study of the Jana Sangh to illuminate. How is Hindu tradition reshaped by being placed in party platforms: How might the Jana Sangh help to formulate a conception of nation and community which would make modernization more accessible to the many new political participants for whom western secularism has few referents: What role does tradition play for the apparently modernizing sectors, students, intellectuals, and urban middle class, who have been attracted to the Jana Sangh—increasingly so recently. Baxter suggests that the cow and the nuclear bomb cannot both be accommodated, that modern technology will bring a secularism which the Jana Sangh opposes—this also I would like to leave as a question.

No study can move in these many directions. Baxter's biography has provided much useful material and opens up a set of questions which many scholars will want to consider—perhaps the most important compliment to any work.—CAROLYN ELLIOTT, *University of California, Santa Cruz.*

The Face of the Third Reich: Portraits of the Nazi Leadership. BY JOACHIM C. FEST. Translated by Michael Bullock. (New York: Pantheon Books, 1970. Pp. 402. $10.00.)

Joachim Fest has written a balanced account of the Third Reich in which he focuses on the personalities of its leaders. He divides his book into three parts, the first dealing with Hitler, the second with those who in Mr. Fest's judgment were next in command (Göring, Goebbels, Heydrich, Himmler, Bormann, and Röhm), and the third with "functionaries" such as Franz von Papen, Hans Frank, and the commandant of Auschwitz, Rudolf Höss. The third part also contains studies of three groups of "functionaries" none of whom, according to the author, had a typical member. These groups are the generals, the intellectuals, and all the women who were mobilized in one way or another into the service of the Third Reich.

Apart from the problem of ranking his subjects according to their places in the Nazi hierarchy, Mr. Fest had to decide whom to deal with and whom to ignore. He tries to justify his failure to discuss certain high officials by saying that their personalities were not very different from those of some of his subjects. But it remains unclear why there is a study of Albert Speer and none, let us say, of Hjalmar Schacht, why Rudolf Hess is dealt with and not Robert Ley, and so on. Also, we are left to derive what we will about two especially interesting groups, the judges and the civil servants, from a general discussion of Weimar conservatism which occurs in passing in the chapter on Papen. One wonders why the generals but not the civil servants rate a chapter. Mr. Fest answers this question by saying that to deal with the civil servants and other groups whom he ignores would withdraw attention from the guilt of the entire German population. But ethical problems aside, why then single out any groups for special treatment?

Yet I spoke of the book as balanced, and the fact that Mr. Fest treats a number of things in passing was one of my reasons for doing so. To cite two examples besides his discussion of Weimar conservatism, he uses the chapter on Bormann to discuss the governmental structure of the Third Reich and the one on Goebbels to deal with Nazi propaganda. The digressions contain nothing less than a summary of the collective wisdom of the social sciences on the structure and the appeals of the Nazi regime. This summary and the studies of individuals and groups combine to make the book a reasonably comprehensive account of the Third Reich.

Other factors add to the balance of the book. Mr. Fest is restrained (and convincing) in his attempts at psychoanalysis. And his concluding remarks about the future of the Bonn Republic could not be more sober, though readers who overrate the importance of the recent decline of the National Democratic Party (NPD) may find his outlook too dreary.

Mr. Fest has distinguished himself in West Berlin and West Germany as a radio and television journalist. Unfortunately, he fails to bring the proper methodological rigor to his book. This lack is most pronounced in his numerous references to ideology. He does not offer an explicit definition of the term. One must invariably read a definition into the context, and the definitions are by no means uniform. Still, the remarks taken together form a most sensible statement on the relations between ideas and political behavior in the Third Reich.

The author does give some thought to the problem of defining "totalitarianism." He says that totalitarianism differs from "the classical forms of coercive government" in its attempt to remake man, and proposes this attempt as its defining characteristic (p. 292). His suggestion recalls one of Hannah Arendt's themes in *The Origins of Totalitarianism.* To be sure, before the definition could gain wide usage agreement would have to emerge on whether all regimes which are considered totalitarian, and these alone, sought to remake man. Mr. Fest does not offer the necessary evidence, or even reveal all his assumptions about what would constitute human re-creation. There is the further problem that the definition might increase the danger, already great in some conservative circles, of overestimating the extent and the causal significance of totalitarian leaders' aims with respect to human nature. But fortunately Mr. Fest does not make this mistake in dealing with the German case, informed as he is about the roles played by Nazi ideology in the various senses of that term.

For sources, Mr. Fest relies mainly on books, journal articles, and the records of the International Military Tribunal at Nuremberg. His most original and enlightening chapters are on individual Nazi leaders other than Hitler. These chapters can be seen as an extension of Miss Arendt's insights into the banality of evil. The part of the book devoted to Hitler is largely a survey of well-known material which had appeared before the German edition was published in 1963. (The notes in the English edition take account of some of the more recent material.) The most disappointing chapter is the one on the intellectuals. It scarcely recognizes the problem of men such as Wilhelm Furtwängler and Werner Heisenberg who, whatever they thought of the regime, continued to exercise their extraordinary talents under it and to lend it their prestige.

Several comments on technical matters ought to be made. Michael Bullock has produced a serviceable translation, though it is an obvious misreading of the original to put down that Göring shared with Renaissance man a supreme sense of style and a refined feeling for life, and Papen's political party (the Center) deserves to have its name capitalized. The publishers would have enhanced the value of the book if they had reprinted the pictures of the subjects from the German edition.

The book is nevertheless the best available introduction to the Third Reich. Especially in Germany, where psychoanalysis has traditionally been more suspect than it is here, the book should raise the general level of ability to recognize political figures with severe emotional disturbances, and that will be a very useful function indeed.—GLENN SCHRAM, *Marquette University.*

The Life and Death of Soviet Trade Unionism. BY JAY B. SORENSON. (New York, Atherton Press, 1969. Pp. 283. $9.50.)

This book, like a great deal else published today, bears out Alexander Erlich's remark in his parallel *The Soviet Industrial Debate 1924–1928,* that "The ideas of the twenties are far from dead in our days." There have not of course been any genuine discussions in the Soviet Union since 1928, and while the country has changed massively since then it is certain that their own experience is more relevant to current Soviet problems than ours.

Erlich's book was an attempt to reconstruct the debate in terms of economic theory; Professor Sorenson is concerned primarily with the politics and institutional structures which helped to produce the modern Soviet regime. As such it is a fascinating study of a certain type of politics—that of a bureaucracy, where there is no recall or referendum to a broad constituency, and no limits placed by cost accounting. The winners are not necessarily "right" in any economic or political sense, but it is they who write the history and determine policy into the future. In the case of the events described in this book it may very well have been Burkharin who was "right," in all senses except the final one, victory.

For many purposes the story ends in 1921 at the 10th Party Congress with the final departure of the Communist Party from any real pretence at a genuinely popular role. The Congress was preceded by the dangerous mutiny at Kronstadt which was crushed by force, and by a long debate on the role of the "unions" in the new Soviet State. The answer was a resolution "On Unity" which left and leaves no room for "factionalism" or any real debate within the Party, much less outside. Lenin himself presented this resolution and so can be considered the author of the Stalinist tyranny, and certainly of Stalinist labor policy.

The rest of the book details how these decisions were implemented, the growing power of Stalin within the Party and the Party within the state. As Professor Sorenson indicates, the basic millenarianism of Lenin and of most of the other Bolsheviks pushed them to decisions which were seldom fully rational, and have proved in many cases harmful to their own goals. The process continues today. Brezhnev, faced with the usual serious crisis in agriculture and a growing industrial crisis, can only ask Soviet workers to go once again into the breach, to fight on to victory, all military metaphors, and not overly applicable, one would think, to what are basically flawed economic structures. Professor Sorenson makes it clear why and when Soviet politics and Soviet economics became hopelessly entwined, so that any reforms on the economic side threaten the power and integrity of the communist Party.

All and all a very useful book. Professor Sorenson does not entirely avoid the usual difficulty of identifying "workers," working class, trade union movements, labor movements, though he does struggle to make clear which he is speaking about and when. This is especially necessary for the early years when the "spontaneous organization of the "working class" seems to have taken the form of factory committees of an anarcho-syndicalist cast. In many cases the Bolsheviks *imposed* trade unions as a method of control. The genuine unions which had roots in the Tsarist period, like the railway workers, became opposed to Bolshevik control and had to be crushed. The whole picture of "labor" is further clouded by the traditional strong ties of the Russian industrial worker to his native village and by the failure except in a few crafts for any sense of class identity to develop.

One does not have to go as far as the late Isaac Deutscher in assuming that an emerging "working class" will come to play a major role as a class in the full Marxist sense in future Soviet politics to develop a feeling that the present Soviet labor union structure has its weaknesses, and that the relationship "labor-unions," and Party-State may be a critical one in the future. It is interesting to note that in the recent (1966) *Industry and Labour in the USSR,* a work by Soviet industrial sociologists, there is no attempt to discuss the role of the trade unions. One suspects no Soviet sociologist would care to comment.

Professor Sorenson also struggles with the subject of historical inevitability vs. the only too obvious Bolshevik manipulations, and comes out strongly for the proposition that the Soviet Regime was the product of a series of accidents. This is of course a hardy perennial, and has been handled better elsewhere.

It is a pity, especially in view of the useful documentation and bibliography, that the book was not provided with an adequate index.—Lois Stone, *SUNY at Albany.*

The Working-Class Tories: Authority, Deference, and Stable Democracy. By Eric A. Nordlinger. (Berkeley: University of California Press, 1967. Pp. 276. $8.95.)

This book is one of a series of writings in recent years about "the working class Tory vote." Nordlinger joins several political scientists and journalists in placing a great emphasis on those members of the English working class who cast their vote upwards to the middle class Tory party. While such a voting pattern surely does exist, it is baffling why, in contemporary English politics, this is considered so important or, for that matter, so unusual. For if one broadens the field of view to also include a look at middle class voting behavior, one finds that not a very much larger percentage of the working class vote Tory than the middle class vote Labour (32% and 20%, respectively). Further, it is significant that these figures were derived using objective class definitions, and, if people's own subjective class identifications are instead employed, then the figures come yet closer together (28% and 21%, respectively). Data taken from Butler and Stokes, *Political Change in Britain,* pp. 106, 76).

Nordlinger again embraces the views of others when he explains much of this working class Tory voting as due to deferential attitudes. For years, citations of "deference theories" have been prominent in the literature as explanations of the working class Tory vote; however, actual survey results have shown that the large majority of the reasons given are not deferential, but rather give the same reasons the rest of the English population gives for voting Tory. The reasons are fairly common and expected ones concerning "bread and butter" issues, government management, and the personality and ability of party leaders (without attempting to construe the latter as "deference"). These reasons are just as valid for the working class as for the middle class—especially if one considers that the Tories were just completing a well-managed and prosperous fourteen year rule at the time of Nordlinger's study in the summer of 1964, and that they had been actively appealing to working class interests ever since the Reform Bill of 1867, as McKenzie and Silver amply document in their book, *Angels in Marble.*

Of couse, there are a scattering of "deferentials" among the working class. But the answers to open-ended questions in surveys show that they are a minor phenomenon. Nordlinger approached his data in such a way—predicated upon an Ecksteinian theory of attitudes toward authority relations —that he overemphasized the "deferential" aspect of working class Toryism, so much so that to him it is the vital key to England's stability as a democracy.

There are several reasons why this overemphasis on the deference explanation resulted. First, Nordlinger did not incorporate the middle class into his sample, so contrasts of its political attitudes and voting behavior with those of the working class were impossible. This distorted his perspective as to the importance and uniqueness of working class voting deviancy and the deferential theory underlying it. Second, he did not place enough emphasis on—or more accurately, he did not derive the vital deductions from—the open-ended questions he asked about the parties, and they provide definite clues as to the place of "deference" versus other explanations the working class gave for supporting the Tories. Third, he defined "deferentials" on the basis of answers to two close-ended questions—presenting structured, forced-choice questions that may have had little relevance to the subject's actual thinking about politics and the parties. (Open-ended questions asking the things people like and dislike about the parties should instead have been used to define who the "deferentials" were—they reveal the spontaneous feelings of subjects.) The fact that only 28% of the working class Tory voters replied affirmatively to two dichotomous questions, and, hence, were classified as "deferentials" in Nordlinger's scheme, should have alerted him that this was not a sizable quantity on at least two counts: one, 28% is a small figure relative to the emphasis Nordlinger placed on it; two, one would expect 25% of the working class Tory sample to choose the two affirmative replies by chance alone.

A last effort by Nordlinger to emphasize the importance of the 28% deferential figure is also questionable in its interpretation. Several questions purporting to tap "acquiescence" to political authority showed somewhat higher values than the 28% figure. But a real problem is in understanding the meaning of these results. Were individuals truly conscious of and acquiescing to the political authority structure, or were they rather manifesting a disinterest in or remoteness from politics in general? If the working class is little involved in politics, it is difficult to impute a general feeling of conscious deference toward the political authority structure typified by Tory rule.

The rest of Nordlinger's analysis basically contrasts the working class Tory "deferential" and "pragmatic" voters (the latter are the 72% residue) with the working class Labour voters, on various social, attitudinal, and behavioral variables. The Tory "pragmatists" are those working class Tory voters who answered the two close-ended questions in any combination other than giving affirmative replies to both. A fourth category in the tables Nordlinger presents in his book is "*all* working class Tory voters," combining the "deferential" and "pragmatist" categories.

Throughout the multitude of tables presented, one is more impressed with the absence of analytic differences between the working class Tory and Labour voters than by their presence. Small differences are the rule and not the exception. In the case of comparisons between all Tory and all Labour voters, it is possible that either (a) the two groups did not in reality differ much on various criteria or (b) they just were not different on the particular criteria Nordlinger selected for his analysis. As for the comparisons of the two separate categories of Tory voters, "deferentials" and "pragmatists," with Labour voters one feels restrained because it is difficult to know what weight to put on the categories as defined. Several of the cross-breaks indicate the volatility of the "deferential" group, and, although there probably are some true "deferentials" in this category, there seems to be a lot of "random noise" here also. The residue "pragmatist" group of the Tory voters and the Labour voters seem to be quite similar—the first being attracted to the Tories for their issues and the second being attracted to Labour for its policies. But essentially, it is socio-economic considerations which probably underlie the vote decisions of these two groups.

One of the several cases in which Nordlinger does not find a relationship is when contrasting age (or "political generation") to his typology of working class Tory and Labour voters. This is surprising since both the McKenzie-Silver and Butler-Stokes studies found a relationship, and, the latter especially, probed it to reveal still other factors than issues which prompted working class Toryism—basically, the socialization of children into Tory allegiance by parents who lived during the time when Labour was not a major contestant on the electoral scene. The strength of such identification increases over time as has been shown in American voting studies, shaping attitudes toward leaders, issues, etc.—without necessarily implying "deference" but rather the intrinsic value placed on psychological identification with a party. These socialized children are now the older cohorts of the Butler-Stokes sample and are largely responsible for the greater percentage of working class who vote Tory than middle class who vote Labour mentioned above. The cross-class voting of the younger cohorts, on the other hand, balances out—the younger set choosing their parties more on the basis of policy and issue reasons.

Other causal factors also seem to have been missed by Nordlinger in explaining working class Toryism—regional differences, religious differences, the "government turnover" philosophy of the people, and the decline of the Liberal party in British politics with its resulting redistribution of the working class vote. Despite Nordlinger's stated intentions, he picked very few relevant variables and engaged in hardly any multivariate analysis. In essence, one learned little from Nordlinger's work about the phenomenon of working class Toryism, the factors affecting it, or the proper perspective in which to place its relevance and importance on the larger British political scene.—JERROLD G. RUSK, *Purdue University*.

Latin American Peasant Movements. EDITED BY HENRY LANDSBERGER (Ithaca: Cornell University Press, 1969. Pp. 476. $12.50.)

Though peasants have comprised more than half of the population of Latin America and in recent years peasant movements have emerged in a number of countries, only isolated case studies have appeared in print. *Latin American Peasant Movements* by Henry Landsberger is the first serious attempt to provide us with a collection of detailed case studies of peasant politics. Moreover the editor, in his introductory essay, has provided us with a thoughtful comprehensive analytic framework for understanding the dynamic factors affecting the emergence and growth of peasant movements. The title of the opening essay, "The Role of Peasant Movements and Revolts in Development" is a misnomer; rather the bulk of the article is concerned with the emergence of peasant movements, the impact of development on their growth and the social-economic and political determinants of peasant politics. The opening essay purports to provide a common framework for analysis, though most of the contributors "deviate" considerably from it, perhaps because some of the essays are derived from doctoral dissertations written independently of this book.

The major value of these essays is found in the abundant descriptive details and the attempt to isolate variables accounting for movement variations. Unlike the impressionistic and discursive essays written by many of the older Latin Americanists we find the beginning attempts to apply quantitative techniques (Powell and Pearson's essays, for example) and in-depth political-anthropological studies.

The countries in which movements are studied include Venezuela, Mexico, Bolivia, Chile, Peru (two essays), Guatemala and Brazil. An excellent concluding essay by Ernest Feder discusses the impact of ruling class repression on peasant movements, an area of research which unfortunately is too often neglected by U. S. political scientists.

As a pioneering effort, the essays in this casebook have a number of defects—both in methodology and in the analysis of the descriptive material.

In his essay "Guatemala: The Peasant Union Movement" Neal Pearson bases a substantial part of his analysis of peasant political attitudes on a secondary analysis of data collected from imprisoned peasants. Data based on interviews which could be political-incriminating and lead to possible physical elimination is hardly reliable. Hence Pearson's assertion that "there was no significant correlation between being informed and membership in any of the organizations" may have been largely a product of the circumstances in which the "interviews" were taking place: interrogation in a jail by a member of the State Department, Office of Intelligence Research.

In their accounts of the activities of the opponents of peasant movements in Brazil, Guatemala, Bolivia, and Mexico there is almost a total absence of discussion of the role of the U. S.—though it is well known that the CIA was intimately involved in reversing the agrarian movement in Guatemala; that the U. S. provided economic and diplomatic support to the military junta that seized power and suppressed the peasant movement in Brazil: that the U. S. provided the financial and technical resources that reconstituted the Bolivian Army which eventually ousted the peasant-based MNR; that U. S. military expeditions invaded Mexico and attempted to destroy the peasant armies, etc.

Powell's account of the "success" of the peasant unions in Venezuela is a gross misrepresentation of its actual accomplishments—which he states in general terms but fails to detail. The great majority of Venezuelan peasants have either migrated to the growing urban slums or remained landless. The majority of agrarian reform "beneficiaries" have been given inadequate technical and financial assistance resulting in their subsisting on isolated plots of land. Furthermore, the power of the large landowners has not been seriously challenged in rural areas; together with a new class of middle class farmer (about twenty percent of the agrarian reform beneficiaries) they devour the larger part of the credits and marketing facilities made available for agrarian development; and the future trend is for these conditions to be accentuated.

A large and significant peasant movement organized by left-wing Catholics—who have since been forced out of the Christian Democratic Party—and Marxists has emerged for the first time in Chile during the past six years. Unfortunately Professor Landsberger chose to discuss a seventeen year old episode involving a defunct peasant union in the South of Chile. While his analysis is suggestive, his attempt in the concluding paragraphs to link his case study up with contemporary political developments is feeble. The members of the Catholic Left which have organized the majority of the unions were in grammar school when the events Landsberger describes took place. Furthermore the Marxist parties have enjoyed far more success in gaining the allegiance of the peasantry than Professor Landsberger is aware of. During the 1964 elections, in the central valley, a plurality of male rural voters cast their ballots for the Socialist-Communist candidate. These facts are hardly indicative of a "crushing defeat."

The account of the Mexican peasant movement by White also exaggerates the degree of success and fails to adequately take account of the enormous number of peasants who have not been materially affected by the revolution. Along with the bulk of the Mexicologists he fails to recognize the emergence of new exploitative classes which have emerged in the last twenty years and which clothe their political rhetoric in the language of peasant revolution while liquidating or co-opting grass roots peasant leaders who attempt to extend the revolution to the present day.

There are occasional intellectual lapses among some of the authors attempting to interpret peasant behavior that reveal some of their prejudices. For example, Powell inelegantly informs us that: "Current peasant choices of agrarian reform benefits and housing over education are perfectly consistent with everything we know (SIC) about the absence of deferred gratification behavior in backward peoples." One wonders who really is "backward": a peasant in search of a home and land for survival or a U.S. professor advising him to defer these "gratifications". . . .

On the other hand the article by Cotler and Portocarrero on Peruvian peasant unions and Cynthia Hewitt on Brazilian peasant movements are more balanced accounts, less influenced by the liberal biases found among the other contributors.

Huizer and Hewitt compile a comprehensive bibliography which will be especially helpful to the specialist largely because many of the entries are not generally available.—James Petras, *The Pennsylvania State University.*

The Unionization of Teachers: A Case Study of the UFT. By Stephen Cole. (New York: Frederick Praeger Publishers, 1969. Pp. 245. $8.00.)

Teacher Unions and Associations: A Comparative Study. Edited by Albert A. Blum. (Urbana: University of Illinois Press, 1969. Pp. 353. $9.50.)

As interest in the politics of education expands, there has been an increased concern with the role of teacher organization. In 1967, Harmon Zeigler wrote *The Political Life of American Teachers,* a study based primarily on a survey of Oregon teachers. Also in that same year, James M. Clark published *Teachers and Politics in France,* a pressure group study. Two years later Alan Rosenthal wrote *Pedagogues and Power,* a comparative study of teacher organizations in five American cities. Now Stephen Cole has produced a sociological history of the United Federation of Teachers (UFT), the nation's largest union local; and Albert Blum has edited a collection of essays on teacher movements in nine countries.

The growth of UFT, now more than 50,000 members strong, is a story worth telling not only because of its implications for American education, but also because of its significance in the development of unions for professionals, particularly government workers. Professor Cole, a young sociologist from Stony Brook, uses two principal research methods: interviews with 22 union leaders and a survey based on mail questionnaires returned by 331 New York teachers after their strike of 1962 (supplemented occasionally by a second survey of 126 teachers in Perth Amboy, New Jersey taken after their strike in 1965).

Until 1960, the history of teacher organizations in New York City was a tale of utter frustration. Fragmented along subject matter, grade level, geographical, ideological and religious lines, the teachers were at one time "represented" by 160 different groups. During the depression, teaching in New York City was one of the best jobs in the country. After World War II, however, neither salaries nor status of teachers kept pace with other professions. Dissatisfaction mounted but the necessary militancy to overcome internal divisions and to take action was slow to develop.

Part of the problem is that the ability to strike is both symbolically and functionally the key to successful union organization. Yet, traditionally, teachers feared to use this weapon, believing that whatever economic gain might be garnered would be offset by a loss of professional and class status. Furthermore such strikes were everywhere illegal.

The UFT was formed in March 1960 by a merger of the CATU, a dissident group of high school teachers, and the Teachers Guild, the remnants of the socialist movement among teachers. Six months later, the new union launched a strike to achieve collective bargaining and in 1962 another strike for wages. As strikes go neither was much of a spectacle. Most teachers crossed picket lines, and in the face of this low support and court injunctions, union leaders called the strikes off after the first day. But collective bargaining was won and the day after the second strike. Governor Rockefeller "found" $13 million in additional state aid for the schools. This meant $1000 a year raise for each teacher plus substantial fringe benefits. It was a lesson the teachers could not ignore and union membership boomed.

Cole's analysis of the strike focuses on two issues: which teachers were most likely to strike and which social conditions make such strikes effective?

Who are the militant teachers? Fifty-nine percent of the teachers in New York's public schools are Jewish as are an even higher percentage of the most visible union leaders. Cole finds, however, that militancy is related less to being Jewish than to family background that was lower-class, Democratic and pro-union. In Perth Amboy, where Jews were more middle-class, Catholics were the most militant. Indeed there appears to be a negative correlation between intensity of religion and union militancy.

Other factors relating to militancy were youth, male sex, and teaching in a high school or even more a junior high. (These latter schools have the most discipline problems and the highest staff turnovers.) The educational philosophy of respondents, if one accepts the questions Cole asked as valid indicators, was not a factor.

Perhaps because of his limited sample size, Cole made no attempt to determine the attitudes of Black or Puerto Rican teachers regarding the strike. Given the current conflict between these minority groups and the UFT and the anti-union stance of the Afro-American Teachers Association, these attitudes would have been an interesting dimension to the study. In the light of these current hostilities, there is a certain irony in the unintentional assist the civil rights movement gave to union solidarity. According to one union organizer:

> The teachers have been very afraid of committing illegal acts, and this is why the civil rights movement has been so important. Without the civil rights movement there never would have been so many teachers' strikes. The civil rights movement has given legitimacy to breaking the law when the law is immoral. The law prohibiting teacher strikes is immoral. . . . (p. 74)

As Cole points out the response of the men on the other side of the bargaining table to the growth of union power was generally weak or erratic. None of the potential techniques of social control—displays of paternalism, token concessions, co-optation of leaders or legal sanctions—was used very effectively. Sanctions, the author suggests, were particularly difficult to apply because of the prolabor stance of Mayor Robert Wagner's administration and because the relevant statute (the Condon-Wadlin Act) called for instant dismissal of strikers, an overkill penalty few authorities were willing to invoke. These are clearly important factors, but Cole's failure to interview members of the school board or other political officials makes this chapter more speculative than necessary.

The most interesting policy question Cole discusses is whether unionization has had a negative effect on the professional interest of teachers in the improvement of schools. On this issue, which is the classic controversy between the National Educational Association and the American Federation of Teachers (the UFT's parent), the author is rather skeptical about the union's position. When there has been a choice between increasing the strength of the organization or advancing educational reform, Cole believes, UFT leaders have chosen the former. While on two issues, salaries and class size, the union has won substantial gains for its membership and education generally; the leadership has also blocked reform of teacher licensing and evaluation procedures. The union has opposed the kind of faculty participation in hiring and promotion decisions common in universities on the grounds that such participation by its members would obscure management-labor distinctions.

This conflict between the well-being of the organization and the desperate need to reform urban schools may have a great influence on the future of teachers unions. Unfortunately, however, Cole's book was completed before the recent struggle over school decentralization in New York City began and he devotes only two pages in an epilogue to the subject. Though the decentralization issue is far from settled in New York and is just now emerging in other cities, the demand for accountability and performance that it represents may in the long run affect the union's ability to advance the economic interest of teachers, guard such perquisites as tenure or maintain internal solidarity. If as Cole found, the young, the poor and the liberal were the most ardent union members a decade ago, these same groups today often view unions as part of the establishment. These educational militants have found a new cause and a new enemy.

Albert Blum, Professor of Labor and Industrial Relations at Michigan State, has collected ten original essays on teacher unions and associations ". . . to better understand the organizations teachers are developing as they make their demands." (p. vii) The volume, arranged alphabetically, includes chapters on Canada, England, West Germany, India, Japan, Mexico, Nigeria, Thailand, the United States, and a final chapter on international teacher organizations. This internal arrangement and the nation-by-nation format suggest that the essays were commissioned and assembled without considering the need for a common analytical framework.

Consequently, the contributions which are also written from several disciplinary perspectives, produce very little in the way of comparative analysis. Many of the essays are unfocused descriptive inventories of the various organizations in each country rather than explorations into the sources and consequences of organizational behavior.

Blum suggests the concepts of professionalism and unionism as an organizing theme, while arguing that a major purpose of comparative study is ". . . to permit us to see our own problems and our answers to these problems from a wider vantage point." (p. vii) Professionalism/unionism may be appropriate categories for analyzing some aspects of the American scene; however, their ideological flavor, American origins, and tendency to reduce the complexity of stances taken by professional organizations, suggests that they alone cannot serve as a framework for the comparative study of teacher organizations.

A preferable approach might have utilized the well-established literatures on organizational theory, industrial relations, group politics (particularly Lipset and Schwartz' essay "The Politics of Professionals" in Vollmer and Mills, *Professionalization*) and comparative political behavior to articulate the goals of research in this field.

A framework for the comparative analysis of teacher organizations must perform at least three tasks. First, it should help identify those variables which cause the diverse patterns of organizational behavior among teachers. Second, the framework ought to provide the analyst with a device for comparing the structure and behavior of teacher organizations. Third, and most difficult, it should provide some criteria for identifying the most important probable consequences of organized activity on the part of teachers. Ideally, the scheme should help us explore the "feedback" effects of organizational behavior on the society and the organization itself. It should also suggest what impact these patterns of behavior have for the governance of education and the quality of education.

Moscow and Doherty approach this ideal in their fine overview of the United States. They argue that the behavior of local affiliates of the NEA and the AFT is probably more satisfactorily explained by variables such as state-to-state differences in collective bargaining laws, alliances with labor organizations, increases in the proportion of men in the profession, competition between the rival organizations, and changes in the level of educational quality rather than by their professionalism or unionism identification. They also suggest that the professionalism—unionism *issue*, linked as it is to the problem of educational quality, cannot be resolved until an ". . . adequate yardstick exists by which one can measure education quality." (p. 331)

They surmise:

> . . . that urban schools have deteriorated in recent years, but it would be foolish to attempt to show a cause and effect relationship between bargaining (a characteristic of unionism) and the decline in quality. . . . In fact it might be easier to show that it was the decline in educational quality that precipitated bargaining, and that without some form of collective activity, the erosion would have been much more rapid. (p. 331)

Although the essay on Canada by Douglas Muir is a substantial and well-written piece, it focuses on the diverse legal frameworks regulating teacher's rights to bargain collectively rather than on explanations of the organizational behavior of Canadian unions and associations. There is only a passing reference, for example, to the complex mosaic of the fascinating teacher movement in Quebec, where associations reflect the linguistic and religious cleavages of the province.

Similarly, Natarajan's chapter on India is also entirely descriptive. The author does not speculate on the possible consequences for the development of teacher organizations of the decentralization represented by the "Community Development Blocks."

Professors Heenan and Wranski in their chapter on Thailand describe a myriad of specialized teacher organizations such as the Pre-School Education Association of Thailand. They devote three pages to a listing of the welfare and continuing education programs of the *Khuru Sapha*, the government sponsored "company union" which all Thai teachers must join, without explaining why teacher organizations in Thailand have not achieved even a minimal degree of autonomy.

Victor Alba's essay on Mexico also fails to explain why teacher unions may have lost their autonomy. Why the teacher unions became largely linked to the Ministry of Education rather than the official party (*Partido Revolucionario Institucional*) is a critical question he left unanswered.

The outstanding contribution to this volume is the chapter on Nikkyōso, Japan's most important and powerful teacher union. Solomon Levine, who authored *Industrial Relations in Postwar Japan*, accounts for the rise and decline of Nikkyōso since the occupation, tracing its relationship to the labor movement, the Ministry of Education, and its ability to adapt to the centralized, decentralized and again re-centralized system of education in Japan since 1945. In what might well have served as a model for the entire volume, Levine's *tour de force* focuses on ". . . those features that appear useful for comparing Japan's experience with other nations." (p. 142)

While Blum's book takes the first step of suggesting that the comparative study of teacher organizations is worthy of our further attention, there is obviously much theorizing and research to be done.

Only eleven years ago, Thomas Eliot in an essay in this Review (December, 1959) accurately complained that political scientists had woefully neglected the politics of education. Since then, however, the number of universities offering courses in the field has grown to at least 44 and reading lists, albeit with some significant gaps, can be assembled for both American politics and the comparative politics of education. Cole and Blum have made useful contributions to these lists and several more comprehensive and theoretical works in the field are on the way.—PAUL M. COHEN, *Teachers College, Columbia University*. GEORGE R. LA NOUE, *Teachers College, Columbia University and Horace Mann-Lincoln Institute*.

Contemporary Yugoslavia: Twenty Years of Socialist Experiment. EDITED BY WAYNE S. VUCINICH. (Berkeley: University of California, 1969. Pp. 441. $9.50.)

This anthology of recent writings by seven contributors grew out of a conference on contemporary Yugoslavia held at Stanford University in December of 1965. Five of these articles were prepared for that conference; two others (on Yugoslav Marxist thought and on Yugoslav modernization) were commissioned. In light of these facts, this volume is surprisingly coherent in its diversity. In short, it represents a solid contribution to the study of contemporary Yugoslavia, and, although flawed, is well worth purchasing. To give some idea of the book's usefulness, I shall comment briefly on some of its weak points and its strengths. First off, how is it "flawed"?

The weaknesses of these efforts taken as a whole consist of being misleading, uneven and occasionally blatantly inconsistent. Like some comely women, this book is very attractive—even though its feet are much too big. The title, that is to say, is misleading, for one does not expect fully one third of the text of a work on "twenty years of socialist experiment" (sic) to survey pre-1945 Yugoslavia. The market abounds with a wide variety of competent works that provide that "necessary foundation." Unevenness is to be expected in an anthology, even among writers such as these seven who primarily are historically oriented. But the inconsistencies seem to indicate that several of these contributors did not take time to apprise themselves of each others efforts. For example, compare Tomasevich (p. 62) with Vucinich (pp. 25, 278) on the significance of the Concordat which was rejected by the Yugoslav parliament in 1937.

Turning to the body of the book, it offers much to inform the observer of modern Yugoslavia. Wayne Vucinich presents inter-war Yugoslavia in an even-handed fashion, setting the heavily historical tone that characterizes the entire book. Occasional lapses into historicism and a conspicuous absence of well-founded generalizations mar a generally profound presentation of Yugoslav political development during that period.

Jozo Tomasevich does deliver (as promised in the preface) "the finest short treatment of the enormously complicated Yugoslav war scene currently available," indicating in passing that Yugoslavia's stormy relations with the USSR began as early as 1942. Noteworthy is his judicious seven point conclusion.

Woodford McClellan's "Postwar Political Evolution" and Phyllis Auty's "Yugolsavia's International Relations (1945–1965)" jointly provide a sensitive analysis of Yugoslavia's domestic and international political metamorphosis during those two decades. As Tito remarked bitterly to Phyllis Auty in 1951, the Yugoslavs have seldom had very satisfactory relations with the Russians. Perhaps this candid comment from the George Washington of Yugoslavia could serve as the implicit theme of this book.

George Macesich is quite disappointing in his efforts to inform the reader of "Major Trends in the Postwar Economy of Yugoslavia." Evidently an economic historian by nature, he immerses the reader in waves of economic statistics that all too seldom are examined for meaning. The facts too often are mute; Macesich describes not wisely but too well, often leaving major tasks of explanation and evaluation to the reader. Most inexcusable, however, is his cursory treatment of what several of these writers admit to being perhaps the most important contribution of the Yugoslavs to Marxist theory and practice: the principle of self-management and self-administration, in both economic and political spheres of activity. Closest to a general appraisal is his opaque statement (pp. 229–230) that

> Strict application of such principles of management to an economic enterprise when coupled with the assumption of income-maximizing worker-managers has been shown to be theoretically inferior to existing practices in capitalist firms. . . . This does not mean that the Yugoslav system is not workable. It does mean that it is not fully efficient in the economic sense.

One wishes that Macesich would join his sometimes astute theoretical remarks to a firm description of the effectiveness of those principles in practice, especially as to how they vary in implementation from one region to another within Yugoslavia.

Vucinich, in his second contribution "Nationalism and Communism" draws the reader again back to inter-war Yugoslavia, and leads him up to date through a historical labyrinth of internecine warfares that strain a Westerner's imagination. Truly, as George Bailey commented (in *The Reporter*, July 1, 1955), Yugoslavia "harbours a family of nations united by the fear that their hatred of each other may be exploited by the outsiders." The Yugoslav communists have been able to surmount those animosities by pioneering in the construction of an independent, unitary (yet federal!) socialist state. Paradoxically, this not only allows for, but encourages real, sustained effort at political and economic decentralization. Although not yet fully achieved, this is a tribute to Yugoslav native pragmatic ingenuity and to their stubbornness in the face of a usually hostile, powerful USSR.

M. George Zaninovich presents "The Yugoslav Variation on Marx" in such sophisticated and persuasive fashion that it almost defies critical comment. This talented theoriest weaves together six threads of discussion into a colorful fabric that displays well Yugoslavia's "interesting and lively theorizing"—warts and all. The struggle of national liberation as a founding act of the Yugoslav state is quite stimulating reading. The immediate post-liberation, heavily Stalinist beliefs are forgotten by many current adulators of Yugoslavia. The basic Yugoslav conception of man and the man-nature relation is another artful exploration in mythology and political anthropology. Very intriguing is Zaninovich's discussion of the Yugoslav theory of state and society, which comprehends such matters as centralism, bureaucracy, etatism, decentralization, and self-government, touching also upon the newly qualified role of the Party. The Yugoslav theory of dynamics and socio-economic change shed light on the "socialist transformation" and the transition to communism. Finally, the nature and implications of the much-touted "young Marx" are examined for insights into current internal criticisms of the Yugoslav regime, as well as into the justifications offered by that regime itself for their rather bold theoretical (and practical) innovations.

This brilliant essay deserves close reading, for it will stand for some time as a major contribution to the study of Marxist theory and practice in general, and of modern day Yugoslavia in particular. The data of this article (as well as all others in this volume) are heavily based on a wide and deep reading of diverse Yugoslav sources in Serbo-Croation. Further, the footnotes are valuable (and quite readable) for following lines of the argument in greater depth.

Logically it would seem that Joel Halpern's finely wrought work of condensation on "Modernization in an Ethnically Diverse State," would follow the Macesich offering in the procession of authors—but it doesn't. Rather, it serves as the concluding piece of this book, perhaps because it leaves the reader content after being simultaneously enlightened and entertained, in the best sense. Halpern's grasp of a wealth of data, presented with great verve, is formidable. His comments, weaving together economics, history, anthropology, geography, education, and political science, happily propel the reader to think about urbanization of the villages and peasantization of the towns, the role of extended families and kin ties in easing the pains of modernization, the emancipation of Yugoslav women, the Eastern European concept of *kultur*, and, most ingeniously, school primers as an index of regional modernization. This book ends on the up beat.

One final note. The excellent index merits mention. Unfortunately the binding, although a colorful and attractive grey and red, broke after one month's use (U.C. publishers, are you listening?). Overall, this is an excellent source of stimulating and carefully considered thoughts on that most deviant socialist state, Yugoslavia. It is well worth reading.—George Wayne Bradley, *San Francisco State College*.

Australian Federalism in the Courts. By Geoffrey Sawer. (New York: Cambridge University Press. 1969. Pp. 262. $12.50.)

The Effect of Judicial Review on Federal-State Relations in Australia, Canada, and the United States. By Richard E. Johnston. (Baton Rouge: Louisiana State University Press, 1970. Pp. 320. $12.00.)

Albert Venn Dicey, the *doyen* of English and Empire constitutional jurists in the golden age immediately before the outbreak of World War I, criticized federalism as meaning, variously, weak government, conservatism, and finally legalism. Geoffrey Sawer, an Australian lawyer, and Richard Johnston, an American political scientist from North Texas State University, address themselves to this third aspect of Dicey's triadic criticism of federalism, for each is concerned with the political impact of the courts on federalism in the exercise of judicial review of the constitution. Both books are comparative in emphasis, Professor Johnston's expressly and Professor Sawer in fact if not in stated design. Any study of the judicial interpretation of the Australian Constitution, of course, could hardly avoid taking note of American experience, since not merely did the Founding Fathers of the Australian Constitution borrow freely from the American Constitution in drafting their own constitutional text, but two of the leading judicial personalities, Isaacs and Dixon, were well acquainted with American Supreme Court decisions and drew freely upon them in their judgments.

Sawer's book is a non-technical book, clearly intended for the general social science reader as much as for law students. In a country that treats its constitution, and even more its constitutional judges, soberly and ponderously, Sawer has provided us with illuminating flashes of personal character and temperament of the individual judges which do much to explain otherwise idiosyncratic or aberrant judicial opinions. The frequent wit and irreverence and also the facility of style in Sawer's handling of the discussion of what are surely the most consistently long and also the most determinedly academic and conceptual court judgments among all the federal countries, may help to introduce a new and much-needed element of American-style Legal Realism into Australian constitutional law.

All the same, one cannot help wondering again after reading Sawer's book, how much Australian federalism, as law-in-action today, is really a response to calls for ethnic-cultural diversity or regional particularism as in other federal societies; and how much, by contrast, it is a purely artificial, lawyer's creation—a sort of play within a play, built upon the scholarly erudition and the conceptualistic elaborations of what must surely be the ablest public law bar in the Commonwealth Countries. Felix Frankfurter used to recognise the degree of logical refinement of Australian constitutional jurisprudence in his remark, repeated more than once to visiting legal delegations, that the High Court of Australia was the World's strongest English-speaking court. It may be a tribute to the resilience of Australian society, if not rather a testimony to its relative un-complexity and freedom from internal strife, that it has survived such a continually rich diet of legal logic—*Begriffsjurisprudenz*—without too much of a political strain developing between the positive law and the society that it is intended to serve. If cases like the *Bank Nationalisation* case of 1948, in the political conflict between a Socialist federal government and a *laissez-faire*-oriented judiciary may seem, on a superficial view, to confirm the old maxim from American Constitutional history that those who can no longer control the legislatures tend to look to the courts as guardians of their special interests and privileges, these cases are easily balanced by the more usual political non-events like the *Airlines of New South Wales, (No. 2)*, case (1965), where a Conservative federal government and a Socialist state government were joined in battle over the power to regulate internal, intra-state, air traffic; and where the court, on a basis of legal logic and without apparent regard to policy, happily upheld both jurisdictional competences, federal and state.

Professor Johnston, for his part, selects a rather wider canvas than Professor Sawer, looking to judicial review of federal constitutions of the United States, Canada, and Australia, under the four main headings—inter-governmental immunity of instrumentalities; taxing and spending; foreign affairs; trade and commerce. He has done a thorough and comprehensive survey of the case law of the three federal systems surveyed. However, making allowances for the breadth of the subject and the comparative brevity of his treatment, I would have two main criticisms. First, in seeking to give us, in effect, a judge's-eye view of each of the three federal constitutions surveyed, Professor Johnston necessarily tends to imply that the institutional emphasis and balance in each of the three is the same, and that all three constitutions are judge-oriented in terms of really important community policy-making. A study of the constitutional law-in-action in each of the three federal systems might indicate, however, that this is just not so. Tax disputes that in Australia and in the United States might reach the courts, in Canada today tend to be fought out in the Balkan War-type skirmishes and ambushes of the regular Dominion-Provincial Prime Ministers' Conferences and in the dependent Dominion-Provincial Tax Structure Committee. The *locus* of community policy-making, in federal constitutional matters in Canada, has tended therefore to shift from the federal Supreme Court (where it was dominant in the area of judicial liberal activism of the 1950's), to other arenas in the 1960's. And this brings us to the second main criticism. In relying, as he does, very largely on secondary, rather than primary, source materials, Professor Johnston is the victim of a certain political time-lag. His chapters, not merely on the taxing and spending powers but even more perhaps on the foreign affairs power, really need supplementing, in the case of Canada, by detailed examination of the federal constitutional implications of French Canada's "Quiet Revolution," inaugurated after 1960. The "Quiet Revolution" has already transformed very many aspects of the working federal constitution of Canada; but it is still essentially untested in the courts, and probably, if the present governmental preferred attitudes (federal and Provincial) prevail, never will be.—Edward McWhinney, *McGill University*.

People vs Politics: A Study of Opinions, Attitudes and Perceptions in Vancouver-Burrard 1963–1965. By J. A. Laponce. (Toronto: University of Toronto Press, 1969. Pp. 219. $10.00.)

The average American is the most closely studied political animal in history. For the last three decades social scientists have been examining how he herds together, which mating calls he heeds, and how he "would vote if an election were held today." In Canada however, such information is limited. Our knowledge about polar bears still exceeds that about Progressive Conservatives.

Many factors have hampered the formulation of scientific explanations of the voting behavior of Canadians. Most significantly, lack of funds and expertise discouraged reliable national samples until the elections of 1965 and 1968. Thus the formulation and validation of theories has been difficult. However, small area studies have proven useful heuristically and for limited tests of hypotheses. The work considered here is the most thorough and imaginative to date. It is essentially an account of the behavior of electors in the federal constituency of Vancouver-Burrard during the federal and provincial elections held from 1963 to 1965. However, Professor Laponce also provides many insights into the voting behavior of Canadians generally.

The primary data sources are two random samples of the constituency. The first consists of 334 respondents (289 were interviewed before the 1963 federal election; 45 were interviewed after because of a previous refusal or non-contact). The second survey consists of 306 respondents, all of whom were interviewed before the 1965 federal election. These studies were supplemented by a reinterview of 115 respondents from the 1963 survey in November 1964, and by another random sample which produced 147 respondents before the provincial election of October 1963. In addition, data from several national surveys by the Canadian Institute of Public Opinion were used to make more general inferences.

One of the most informative parts of the study concerns voluntary non-participation. In his first survey of 1963, Laponce found four types of people who chose not to vote in the election. The first type he calls boycotters. Comprising .6 percent of the potential electorate, they refuse to vote because they are opposed to politics generally. Another 2.4 percent of the potential electorate are classified as retired voters. These are former participants who no longer vote because of their age or health. A third group are called barbarians (8 percent). These are mostly poorly educated females who have little knowledge of politics, a poor perception of campaign issues, and little interest in voting. The fourth type of non-voters are called spectators (6.7 percent). They are mostly men who tend to abstain for political reasons even though they are better informed than the barbarians.

Laponce also presents some interesting findings on non-voting in the federal election of 1965. He shows, for example, that between 1963 and 1965 the level of non-voting among well-informed Liberals increased at a much greater rate than among poorly-informed Liberals. A common explanation of this phenomenon has been that well-informed Liberals abstained because they were cross-pressured by the unspectacular performance of Lester Pearson, or because none of the parties were particularly attractive (joint avoidance). However Laponce shows that Liberal voters and Liberal abstainers gave almost the same high ranking to both Pearson and the Liberal party. In addition, Diefenbaker and the Conservative party received almost the same low score among these two groups. If anything, turn-out among the well-informed was higher for those who were cross-pressured than for those who were not (p. 43).

Another set of interesting findings concerns the impact of different types of issues on the voter. Laponce defines two types of issues. Private issues are those which are already of concern to the voter, whereas public issues are those which the campaign brings to his attention for the period of the election. Private issues have greater impact on the voter, particularly when parties discuss them in the campaign. In contrast, public issues which do not become private fade away once the campaign is over. Evidence of these differences comes in part from a reinterview of 110 respondents from the 1963 survey, eighteen months after the election. More people remembered "pensions" as an issue than had mentioned it during the campaign (an increase from 9 to 18 percent), whereas "defence and nuclear arms" dropped from 62 to 20 percent (p. 82). When respondents in the 1965 survey were asked to recall the issues of 1963, 10 percent said "pensions" but only 4 percent remembered "defence and nuclear arms" (p. 82).

After analyzing voters' opinions about the parties and leaders, Laponce examines how voters change allegiance to other parties. He begins by using CIPO data to show that in Canada generally a party can expect to lose between 5 and 20 percent of its electorate at each election. He then turns to his Burrard data to examine the pattern of electoral migrations between 1962 and 1965. Migrants followed one of three pathways. Temporary migrants switched and then returned to their original party, one-step migrants switched and remained with their new party, while two-step migrants supported a different party in each of three federal elections. Next Laponce investigates if voters follow an ideological continuum when they switch parties, preferring those parties closest to them on the continuum. He finds that the NDP-Liberal-Conservative-Social Credit ordering produces the most consistent pattern of changes but still does not account for between one quarter and one third of the transfers. He concludes the work by assessing the value of various socio-economic variables as indicators of partisanship, and shows which characteristics distinguish the supporters of each party.

Like the competent confessor, no reviewer examines a book and finds it without sin. The main weakness of this book involves explanation. While the author says much about which people vote or how they change parties, he says little about why they do so. Part of the blame rests with the sample size. For example, the small number of respondents makes it difficult to relate issue orientation to electoral migration in other than a cursory fashion. However, the author is also at fault.

Professor Laponce begins by stating that this work is essentially concerned with measurement, not with formulating and testing a theory, or with relating variables in a systematic experiment (p.x.). Throughout his work therefore, he introduces concepts through analogies or hypotheses and then presents relevant data. Unfortunately, however, the significance of the analogies is seldom pursued once the data is presented, and while the hypotheses are tested adequately, they explain little since they are not derived formally. Thus, while the author is concerned with measurement rather than with theory, it is often difficult to assess what he is measuring. This is particularly true in his use of typologies where the conditions for classification tend to be arbitrary.

Despite these weaknesses however, the book is well worth reading. Students of Canadian politics will find it particularly valuable, not only for what it says about Canadians, but as an example of how to conduct significant research at the local level in a careful and inexpensive manner.—Wallace D. Gagne, *McMaster University.*

The Politics of Tradition: Continuity and Change in Northern Nigeria, 1946–1966. By C. S. Whitaker, Jr. (Princeton: Princeton University Press, 1970. Pp. 563. $13.50.)

The Politics of Tradition calls to mind Martin Landau's thoughtful critique of contemporary political science:

> Restless, uncertain of direction, anxious about our status, we continue to reach out to other domains for concepts and images. Any new language suggestive of scientific yield bids fair to be transported into political science. Hence, it is difficult to fathom Robert A. Dahl's statement that 'the impact of the scientific outlook has been to stimulate caution, rather than boldness, in searching for broad, explanatory theories.' On the contrary, in the last fifteen years, we have transferred theories with a boldness that defies scientific caution. We have done exactly what Dahl has urged: we have introduced 'broad, bold, even highly vulnerable general theories,' as well as theories of lesser order. Those who embrace scientific perspectives have done this more explicitly; those who don't, more implicitly. Our problem, accordingly, is not a lack of theory—even general theory—but an abundance of theory. We possess such a vast number of theories, models, concepts, schemes, frames of reference (these are the terms we use) as to make one dizzy. But even more, all appear to be impregnable to the erosions of experience. They have enormous staying power: few, if any, are ever discarded. And when we become impatient over their limited yield, someone is sure to suggest another expedition into 'seemingly unrelated disciplines' to bring back 'new ways of looking at things' i.e., a new language. ("Due Process of Inquiry," *American Behavioral Scientist,* IX, 1965, p. 6.)

Perhaps the most striking examples of incautious borrowing from a cognate discipline can be found in "theories" and "models" which purport to explain sociopolitical change and modernization. The intellectual debt owed by Riggs and Apter, among many others, to the sociologists Weber, Tonnies, and Durkheim needs no elaboration here. It is sufficient to note that the payoff for political science continues to be meager, indeed. Why? C. S. Whitaker, Jr. addresses this question in his impressive case study of continuity and change in Northern Nigeria.

Whitaker enters two basic objections to the literature under discussion. First, he observes that theories and models of sociopolitical change tend to be organized around a concept whose applicability is not universal: modernization. According to the author, that concept rests on the problematic assumption that societies undergoing substantial change are somehow destined to follow the "progressive" course of Westernization. Put differently, the concept of modernization bears the stigmata of evolutionary determinism and ethnocentric bias.

Second, Whitaker observed that the concept of modernization is frequently associated with a dichotomization of social characteristics which need not be mutually exclusive. Typically, dichotomized social characteristics—for example, achievement-ascription, specificity-diffuseness, and universalism-particularism—are arranged in a typology of "traditional" and "modern" societies; the error of reification is committed when nominally "ideal types" are made to subsume concrete cases. Whence came the image of mutually exclusive elements in contrasting "traditional" and "modern" societies? From ideal types propounded by the aforementioned sociologists, among others: traditional versus rational authority, *gemeinschaft* versus *gesellschaft* society, mechanical versus organic solidarity, and so forth. (It is worth noting here that sociologists and political scientists generally fail to heed the following rule pertaining to ideal types: since ideal types are intended to have an explanatory function, they must be treated as theoretical systems with empirically verifiable hypotheses. The familiar refrain that they are intended instead to illuminate divergence from empirical fact betrays unsound theory and methodological error. This point is discussed further in Carl Hempel, "Typological Methods in the Social Sciences," in Maurice Natanson, ed., *Philosophy of the Social Sciences,* 1963, pp. 210–230.)

In sum, Whitaker rejects the notion of a necessary relationship between sociopolitical change and modernization. Moreover, he rejects the idea that an initial innovation must stimulate further innovation in a "eurhythmic" pattern. According to the author, notions of "eurhythmic" change and dichotomized social characteristics together produce the expectation that in the contact between Western and non-Western societies the latter will totally accept or reject modernity. He prefers a conceptualization of social change without reference to modernization, one that calls attention to the possibility of "creative adaptations to change, [i.e., to] action that succeeds in utilizing or manipulating new or alien elements to serve established ends and values (and vice versa)." (p. 12) Drawing on data from the emirate systems in Northern Nigeria, Whitaker argues persuasively that " a stable symbiosis of modern and traditional elements" (p. 467) is more likely to result from contact between Western and non-Western societies than a total displacement of traditionality by modernity.

Space limitations preclude a thorough examination of the data presented in *The Politics of Tra-*

diation. Accordingly, I would merely call attention to a section of the study which lends particularly strong support to the notion of "stable symbiosis": the chapter on "Popular Elections and *Neman Sarautu.*" Therein Whitaker analyzes the complex process by which parliamentary government and popular elections were adapted to the traditional practice of *neman sarautu* (Hausa: "pursuit of office and title"). Indigenous elites, often educated in the Western manner, used the novel institutions (1) to defend class interests, and (2) to galvanize political competition for both traditional and "European" offices. Analogues to this situation can be found elsewhere in Nigeria. For example, my own research in Idoma revealed that popular elections introduced at the district level by the British were adapted to the traditional "game" of lineage competition for the clan headship.

I shall conclude by taking exception to one aspect of an otherwise solid book. In the final chapter, the author observes that ambivalence toward factors associated with modernity was reflected in the actions of Northern political leaders. Here is one example: in order to execute the social and political role expectations which were held for them, district heads and other high Native Authority officials were forced to violate the British norm of financial integrity; public funds were often used by these officials for private purposes. (p. 463)

Studies of role conflict in Subsaharan Africa are replete with similar cases. Unfortunately, neither previous investigators nor Whitaker probe the relationship between role conflict and ambivalence; indeed, they do not even define the latter concept. Their claims of leadership ambivalence are necessarily weakened by the lack of a conceptual definition and hard data. I have observed elsewhere that ambivalence—defined as difficulty in choosing behavior from among given role alternatives—need not accompany the perception of role conflict. Empirical data obtained by me in Idoma revealed the following: Native Authority officials experience far less ambivalence in role conflict situations involving factors associated with modernity than is generally assumed to be the case ("Methodological Considerations in the Study of African Political and Administrative Behavior: The Case of Role Conflict Analysis," *African Studies Review,* April 1970.)—ALVIN MAGID, *State University of New York at Albany.*

India's Static Power Structure. BY J. D. SETHI. (Delhi: Vikas Publications, 1969. Pp. 212. Rs 25.)

Professor Sethi is an economist trained at the L.S.E. and at present teaching at the Delhi School of Economics. His book is a casual and discursive collection of essays on current Indian politics written for several leading Indian news magazines between 1966 and 1968. Unfortunately, the value of such a contribution depends largely on the extent of the author's "inside knowledge" of decision making and his ability to articulate his experience in concepts which are precise and current. Professor Sethi's analysis has the mark of an educated and informed newspaper reader but lacks the buttress of access or method and suffers as a consequence. Likewise, although he is familiar with much of the current scholarly literature, he is frequently enticed into an ideological idiom. The book is, nevertheless, interesting as an articulate indication of the response of India's urban intellectuals to the political events following Shastri's death.

The theme of Professor Sethi's book is stagnation in Indian politics. The stagnation to which he refers is the obvious decline in executive stability and authority in India since the 1967 elections and the consequent immobilization of governmental initiative. Symptoms of this immobilism are mentioned in passing as increased communal violence, economic crisis, fissiparous tendencies within the nation, and what he interprets as increasingly erratic governmental responses to a variety of problems from bank nationalization to allocations of grants to universities. The causes are to be found in the cultural betrayal of India's leading intellectuals and the venality of her politicians. The intellectual leaders of the nation have lost touch with their culture, its values and sentiments, and are ignorant of both the limitations and potential of their society. They have produced a "Nehruvian consensus" which is in essence Western and stand here accused of intellectual treason—of looking to the West rather than to their compatriots for meaning, approval, direction and money. The political parties, which might have been expected to resist these tendencies, have failed because the factionalism of self-seeking politicians has incapacitated them for their vital functions of linking elite and mass, evolving a true national consensus, institutionalizing autonomous executive leadership, and imposing normative restraints on the struggle for power. Indira Gandhi, Sethi argues, compounds her father's intellectual guilt by exacerbating his institutional decay. She has adopted, he explains, a deliberate strategy of destroying the only viable national political organization in India, the Congress Party, because she cannot control it. Her vicious attack on Morarji Desai, a major rival, concomitantly humiliated and alienated one of the few top leaders seriously committed to the party and national government as institutions. She has destroyed the integrity of the party with appeals to factions within the opposition parties in the name of "national consensus." She has weakened the central government by wooing state leaders, the *quid pro quo* being justified by the principle of "decentralization." She has dissipated resources and stimulated insatiable demands by indulging in "populist" appeals to the masses. Her success will mean, he wrote several months before the Party split, the destruction of the Congress.

Sethi's preference, one hesitates to call it a solution, seems to be a xenophobic nationalism reminiscent of the thirties rather than the liberal internationalism of the Nehru tradition. Politically, he prefers a functional division of powers within the Indian federation with priorities and responsibilities clearly established by the central government. India's problems, he argues, can be solved only by strong leadership which keeps the nation's energies focused on economic growth through high investment rates. Concern over India's diversity and consumption needs is not unreasonable, it should merely be reduced to its proper proportions.

Sethi's arguments are not implausible, nor are they unique. In fact, in spite of his criticism, one would find these sentiments generally acceptable and frequently articulated within these same "intellectual" circles. His frustration is the fate of a nationalist middle class which has believed the myths of its nationalist leaders and now searches for the culprit who has destroyed natural community of sentiment and interest of the Indian people so widely proclaimed during the freedom struggle. Personally, Sethi has also, perhaps unconsciously, accepted the myths of western nationalism which hold up the socially integrated, culturally homogenous nations of Europe as a model for Asia and, intolerant of pluralism, he sees as a fatal flaw that diversity which may see as the essence and vitality of India's unique cultural tradition.

Sethi's frustration with the immobilism and factionalism of party politics is a recurrent theme among Indian intellectuals. It is unfortunate, however, that he did not bother to analyze the articulation between polity and economy so that one might better understand the precise areas most effected by political crisis. One is given the impression that nothing happens in India without government initiative and wonders, in consequence, about the origins of the "green revolution" amidst all this political confusion. Likewise, it is important to distinguish between elements of economic policy and elements of political expediency in the analysis of decision making as it is to distinguish the influence of economists and administrators from that of factional leaders.

Sethi's conclusion that Indian politics are bewildering and stagnant, expressed in the term "factionalism," is an all too common escape from rigorous analysis. In the first place, he either ignores or dismisses such concepts as "ruralization" or "sanskritization" as devices for ordering his data and for translating factional confusion into aspects of structural change. Second, there is no mention of the rise of lower caste leaders to elite positions, the political decline of Brahmins or the entry of new regions into politics. Certainly, India's power structure cannot be considered static in the face of such momentous developments. Finally, institutional restraints on the factionalism of the party's recruitment process continue to operate. The influence of the civil service persists, non-factional paths are available to Ministerial posts, and highly independent Congress dignitaries occupy positions such as Governorships or directors of national industries. The spillover of factionalism into the actual process of decision-making has simply not been demonstrated.

One final point of interest, the volume is dedicated to the memory of C. Wright Mills.—Norman K. Nicholson, *Northern Illinois University, DeKalb.*

Science and Technology in British Politics. By Norman J. Vig. (Oxford: Pergamon Press, 1968. Pp. 190. $6.00.)

The content of this case study is largely addressed to the "science policy debate" that engaged Britain's major political parties during the period from 1959 to 1966. Relying upon government records, party documents, and forty interviews with leading political and administrative officials, the author has carefully addressed himself to two broad areas of concern: an account of the specific events that affected the emergence of "science policy" in Great Britain and an effort to see the entire phenomenon as a reflection of general political processes steeped in partisan difference and competition.

The emergence of science policy is handled in three chapters that deal (1) with a brief (but incisive) description of civil science programs before the Conservative Government of 1959–64 (2) with policy decisions on civil science by the Conservative Government of this period (3) and with recent attempts by the Labor Government to shift scientific and technological resources from military defense to applied civilian research production.

The author suggests that science policy matters prior to the Conservative Government of 1959–64 were complicated and intensified by several problems in science and technology that began to emerge during the aftermath of the Second World War, although many of them were rooted in older historical practices. Briefly stated, these problems centered around the shortage of scientific and technological manpower, the backward state of applied research and development in British industries, and the necessity to create new forms of government organization for planning and administering support for civil science and development.

The manpower shortage is attributed in large part to the reluctance of British universities to develop and stimulate scientific curricula and research opportunities until well into the 20th century. Yet even in those cases where pure science was found acceptable and research laboratories established, applied science and technology were regarded as inferior vocational or mechanical competences. Thus, a whole tradition of class bias and "classical" scholarship had to be mitigated before shortages in scientific and technological manpower could be approached as a serious matter of Government policy.

In British industries applied science and technology were also neglected—in some measure because of the educational pattern of the universities—but also because the English manufacturing system was built on industrial processes and management concepts that had their origin in the early industrial revolution. From this perspective basic scientific findings could scarcely be considered as important resources for industrial production and process innovation.

It was only in response to severe industrial shortages produced by the First World War that attention was drawn to the prospect of government support for increasing Britian's long-range economic capacity through scientific research and development. The main instrument for achieving this objective was the creation of the Department of Scientific and Industrial Research (1916) which assumed responsibility for a number of operations: funding post-graduate work in basic research, allocating research monies for projects and equipment, stimulating industrial research through matching grants for cooperative Research Associations, and establishing and maintaining a network of State research institutions. The formal decision-making authority for the Department of Scientific and Industrial Research was committed to the hands of a Privy Council Committee composed of ministers from departments with related interests. But in practice it was loosely supervised by the Lord President of the Council who was responsible to Parliament. Informal direction and encouragement was given to the Department by an advisory council consisting of representatives from the universities, industry, and government.

While the decision-making activities that surrounded the DSIR and its advisory panels became the basic administrative pattern for civil research into the 1960's, there were other forms of government organization that shared as well in the cultivation of scientific research. The Medical Research Council (1919) and the Agricultural Research Council (1931) were each founded to promote and coordinate rather specific scientific concerns, but in neither instance was a separate department created. They were operated instead under the Lord President and a system of advisory councils similar to those of the DSIR. Moreover, there were a number of administrative and service departments that maintained research facilities prior to the Second World War, such as the Ministries of Agriculture and Fisheries, Fuel and Power, Health, the Post Office, the Colonial Office, the Admiralty (oceanography) and the Air Ministry (meterology).

This decentralized and largely autonomous pattern of civil science was directly affected by the exigencies produced by the Second World War. Scientists and other professionals were drawn into government administration and research programs were developed (particularly atomic energy and aviation research) that would heavily influence post-war policy. And yet, in general, the wartime application of science was focused on short range goals and lacked sufficient organizational mechanisms to make it a coherent area for overall policy determination.

By 1959 the failure to "modernize" essential industries and to give British commerce a competitive edge forced the Conservative Government to consider additional means through which science could be brought to bear upon national economic policy. Yet the activities of the Conservative Government were limited. It created a new Minister for Science and accepted the utility of the civil development contract. But its operational policies as well as its announced political commitment were against detailed government control and planning for science.

The present Labour Government, however, has shown a disposition to the contrary. It has tried to shift scientific resources from defense to civilian production and in the process has created new forms of government organization for coordinating science-related policies. The Ministery of Technology, for example, which was created by the Labour Government is now being converted into a far more powerful Ministry of Industry for extending control over a wide range of industrial engineering. Moreover, manpower needs in science and technology are being met by incorporating Colleges of advanced Technology into the university sector, and programs are underway for sponsoring industries in computer and machine tool production. But despite this increased activity, the author is insistent that a "national science policy" does not yet exist in Great Britain—in terms of a systematic, costed and budgeted plan for all phases of science-related Government investment.

The second broad concern to which the book is addressed deals with the circumstances that permitted science policy to be exploited as a partisan political issue. Five chapters are devoted to a discussion of the major participants (parties, Parliament, and interest groups) that sought to articulate a national position for the development of science and technology. It is very much to the author's credit that he has explored the external political dimensions of science and brought his analytical facilities to bear on the emergence of science as an instrument of national political purpose. This aspect of science reflects perhaps the most important characteristic of all post-industrial societies—the general collapse of meaningful distinctions between the public and private realms, between sustained support for an autonomous science and the realization that national goals may require the extended use of scientific resources. As the author points out, the participants in the "science policy debate" did not disagree over the political ends to which science might be subjected, but rather over the pace and means by which they might be accomplished.

Science and Technology in British Politics is a work rich in detail, with due caution exercised over what may be inferred from the events described. It will be useful for anyone concerned with comparative science policy or who wishes to speculate about the relation of science to society. My only disappointment (a purely idiosyncratic one) is that the author did not choose to explore what the "politicization" of science might mean for the conduct of scientific inquiry—how it might influence the selection of relevant areas of research or perhaps even the evaluation of scientific evidence itself. But surely an author has the right to establish the parameters of his study, and within the limits imposed upon the present work, Professor Vig has given a competent and worthwhile performance.—MARLAN BISSETT, *Purdue University.*

Modern Capitalist Planning: The French Model. BY STEPHEN S. COHEN. (Cambridge: Harvard University Press, 1969. Pp. 310. $8.50.)

Although the development of French planning has generated an abundance of studies and research, its political aspects have often remained unexamined. This book addresses itself, in part, towards meeting this need. To the extent that it does so, it is possibly the best book on French planning currently available to English-speaking political scientists.

French planning works, the author points out, because of its non-coercive approach. The planners seek to achieve their objects largely through dispensing information and incentives. In the latter category, are such items as cheap investment capital, accelerated depreciation allowances, special tax reductions and outright grants. Consequently, while French businessmen accepted planning only as an alternative to nationalization, many of them have since become outright enthusiasts.

The planners, notes Cohen, have also shown political adroitness at keeping opposition to a minimum. The Planning Commission has purposely remained small and acts chiefly "as a broker among power groups, of which big business and the Ministry of Finance are by far the most important." Another helpful factor has been the emergence of a new breed of *hauts fonctionnaires* who have been intensively and broadly educated at France's postwar École National d'Administration. They bring to their jobs a broad perspective of the national interest as well as an "old boy" network of mutual school associations which helps overcome interagency rivalry and distrust.

These new bureaucrats see themselves as representing the general interest, a concept which, Cohen says, is "straight out of Rousseau." Indeed, it is. However, political theorists will also see a good deal of Saint-Simon in this technocratic meritocracy.

When it comes to the actual mechanics of planning, Cohen makes clear that the various "modernization" commissions, to say nothing of the Economic and Social Council, are not usually the main levers of action. It is more customary for top level bureaucrats to reach an accord with a key industry leader and for the latter to get his colleagues in line. Sometimes no agreement is reached and the industry or any of its members will undertake projects without the government's approval. However, the fact that the planners can offer incentives and the fact that they usually share a common background with business leaders keeps disagreements to a minimum.

Trade unions, small businesses and other groups play only a minimal role in the planning process but, at least to some degree, this is their own fault. Not only do trade unions, for example, often not possess the expertise to make a solid contribution but their leaders, particularly those who are Communists, are hostile to the whole planning process. The same factors partly explain Parliament's inability to play a significant role in planning. The non-Communist left tried to slash military expenditures and increase consumer spending *and* investment in the Fifth Plan in 1964, but the Communists withheld their support. As a result, the "counter plan" fell through and the stage was set for the "events of May" in 1968.

Although Cohen does take note of these and other political developments, political scientists may wish for more data and analysis of the impact of planning on party policies, political campaigns, interest group activity, etc. Furthermore, even when it comes to discussing the way planning is carried out within the bureaucracy and in the bureaucracy's relations with business, his book lacks the detail and specifics needed to see the actual workings of the process.

Some may also quarrel with Cohen's contention that economic planning must inevitably make France into a "corporatist" democracy with no possibility of any significant redistribution of economic power. The participation movement is currently gaining ground in France as well as other European countries, and as knowledge workers replace industrial workers, this trend may acquire increasing momentum. Cohen's own material indicates that a less revolutionary and more reformist trade union movement could wield much more influence over a plan's formulation and execution.

Cohen's book raises, at least in this reviewer's mind, some questions regarding our own discipline. Political Science has produced its share of books on the ill-fated Fourth Republic, but few, if any, of these works have devoted more than cursory attention to what appears to have been the Republic's most significant governmental contribution. Has Political Science become so preoccupied with some elements of the political process that it is neglecting others?

Political Science has also made increasing use of sociological data in recent years. In the light of Cohen's book, the question arises if it is not time to start devoting more thought to economic interrelationships as well?

Finally, the study of Comparative Public Administration has been flourishing of late. But, since it is mainly concerned with cross-cultural comparisons, its scholars have tended to devote their efforts to studying under-developed nations and other countries unlike our own. Cohen's book makes one wonder whether it might not be time to channel more academic energy into studying industrialized lands so that American scholarship may yield more ideas as to how the American polity can cope with its own rising tide of politico-economic problems?—George E. Berkley, *Northeastern University.*

The Government and Politics of Tibet. By Ram Rahul. (Delhi: Vikas Publications, 1969. Pp. 160. Rs. 20.)

This book gives brief accounts of the political history of Tibet, the structure of the Lhasa government before the Chinese Communist takeover, and the place of Tibet in the politics of Asia. The author claims—a claim supported by The Fourteenth Dalai Lama in a "Foreword"—that his is the first book to appear on the subject.

The author's subject, however, has several aspects. The history of Tibet and accounts of its relations with the great powers have been treated more thoroughly in numerous works. The social base of the Tibetan political system has been covered more comprehensively by Carrasco, although he had only secondary sources. What is new in Professor Rahul's book is a reasonably detailed description of the principal offices of the Lhasa government and of their formal functions and powers, and suggestions regarding the political power of various elements of the polity, such as the monasteries, the nobility, and the highest lamas. The combination of this new material with the historical summaries produces an interesting and original introduction to the government and politics of Tibet.

For readers familiar with other "traditional" political societies—roughly, literate agrarian societies with low levels of technology—the material on governmental organization and political power will be suggestive and should help them gain perspective on the political life of traditionalism. This material, however, is too limited to provide more than hints about how the Lhasa government itself really worked, where power really lay, and what policies the government actually pursued. For example, the author says that the Dalai Lama "has full power over the country in all matters, and his work is absolute law" (p. 10), yet he also reports that from the seventeenth to the twentieth century no Dalai Lama lived for more than twenty-three years, that there were extended periods (such as from 1933 until 1950) when the Dalai Lama was a minor political figure and the government directed by a regent, and that even the great Fifth Dalai Lama periodically and for years at a time devoted himself "exclusively to religious affairs" (p. 11). An autocracy frequently without its autocrat requires explanation. Of special importance is the relationship between the religious and political roles of the Dalai Lama, a relationship the author does not examine.

Since, however, the Lhasa regime in its pre-Communist form has disappeared forever, any information on it is welcome. Professor Rahul should be encouraged to turn his attention to those official documents that were brought from Tibet and to former officials of the regime in order to discover what the Dalai Lama's government did, the methods it used, and the goals of its actions.—C. W. Cassinelli, *University of Washington.*

The Christian Democratic Party in Chile: A Study of Political Organization and Activity with Primary Emphasis on the Local Level. By Giles Wayland-Smith. (Cuernavaca, Mexico: Centre Intercultural de Documentación, 1969. Pp. 314.)

In the 1960's, Christian Democracy surged forth as a dynamic alternative to the traditional, Cuban, and Nasserist military approaches to governance. Above all, its remarkable electoral success in Chile in 1964 and 1965 seemed to some observers to presage a continent-wide movement of major proportions. Whatever the ultimate verdict on that score—and it is somewhat dim at this writing—an outpouring of publications focused on the Chilean phenomenon. In large measure, they concentrated their attention on the PDC's national organization, doctrine, and performance level in government.

While not ignoring the national context in this study, Professor Wayland-Smith has chosen to stress the otherwise largely neglected local party units by probing Cristian Democratic organization, goals, recruitment process, and performance levels in three Santiago communes. His data was compiled by interviews and by the use of a questionnaire; his analytical approach is broadly structural-functional.

Three preliminary chapters ably trace the rise of the Christian Democratic party in Chile, its ideological tenets, and the political environment of the Santiago communes the author has taken for his sample: lower class Conchalí, mostly middle class Nuñoa, and well-to-do Las Condes. In this regard, it is to the author's credit that he offers sharp images of the visual features and life styles of each commune so that the empirical evidence that follows is located in recognizable settings.

Politically, all three communes comprise a cross section of Chile's multiparty electorate, with the Christian Democratic percentage of voters being almost identical in each commune. The PDC's local organizational strength built up gradually during the 1950's and 1960's at a time when the other parties suffered from weaker communal organizations —a significant factor in Eduardo Frei's electoral triumph in 1964.

Although the communal parties clearly are creatures of the national PDC organization, they also reflect in structure, membership composition, and attitudinal patterns the individual nature of each commune. Thus, in its organizational form, Nuñoa represents almost an ideal local grouping in terms of what is called for by the party's bylaws. It is highly developed, its headquarters serving as a regular meeting place for all kinds of Christian Democrats in the area—and strong ties have been forged between local leadership and the bases. In contrast, Las Condes constitutes more of a paper organization with a leadership nucleus and virtually no structural bases. Conchalí falls in between the other two communes in its structural makeup. This diversity also exists in doctrinal orientations. All recognize the same general goals for restructuring Chilean society, though the lower class areas prefer immediate, tangible ones, while the middle classes' attachments are more toward global and futuristic achievements.

When considering socio-economic characteristics, Christian Democrats rank "above average" in education, religious commitment, occupational standing, and income, though individual communes vary according to certain criteria (e.g., a higher percentage of women adhere to the PDC in the poorest commune than in the others). The somewhat elitist nature of Christian Democratic membership—mostly middle class but not wealthy —conduces to "a communality of interest and perspective which yields both cross-communal social cohesion and an internal pressure tending to reinforce the specifically ideological and radical contours of the party" (pp. 5/14–15).

In general, the communal parties' activities are similar, but they differ in emphases. Conchalí carries on many service functions, focuses heavily on organizing mass rallies, and relies on a highly pragmatic style. The Las Condes grouping stresses electoral matters, focuses on inner-directed activities that reinforce membership commitments, and is globally ideological in style. Nuñoa stresses service activities and is mixed in its focus and style.

Wayland-Smith concludes that whereas lower class groups can be kept more or less satisfied with the PDC if they feel their claims are reaching the government and a few tangible benefits materialize, middle class elements owe their allegiance to the PDC primarily for ideological reasons rather than for personal gains. As a consequence, they "may undergo a period of waning commitment if global goals become mechanical or subverted or if a sense of emergency declines" (p. 7/12). Although the author does not deal with the factionalism which plagued the Christian Democrats during the last years of Frei's regime, his thesis helps to explain this occurrence. Finally, he suggests that the Christian Democrats, like the Marxist parties, have successfully brought lower class elements into the political system and cultivated working class leadership to sustain the organizational bases. However, in contradistinction to the Marxist parties, the Christian Democrats' political socialization and professionalization of the lower classes has established "a certain foundation for the development of a political system in which conflict leads not to open rebellion but rather to a more equitable and peaceful accommodation of opposing interests" (pp. 7/16–17).

Professor Wayland-Smith has produced an unusual and thoughtful book. He is quick to point out occasional limitations in his data which cause some findings to be suggestive rather than definitive. Still, other research—including that of this reviewer—tend to support his conclusions. Certainly, some interesting and important new dimensions of the Chilean Christian Democratic party are provided in this fine study.—BEN G. BURNETT, *Whittier College.*

The Growth of a Party System in Ceylon. BY CALVIN A. WOODWARD. (Providence: Brown University Press, 1969. Pp. 338. $8.50.)

When independence came to Ceylon, many people tended to see Ceylon as an example of what were considered to be the positive aspects of British colonialism. The island seemed prosperous and the problems associated with high rates of population growth such as unemployment, pressure on the land and demands for services such as education did not diminish the euphoria of freedom. Eventually, the euphoria vanished and the problems remained.

Ceylon's problems are very much like those of any developing state; Ceylon, according to Professor Woodward, "has made the transition to self-rule perhaps better than any other new state." Among the factors responsible for this happy state of affairs are "competent and dedicated leaders" and a "responsible party system." Professor Woodward analyzes the origins and growth of this party system in the work reviewed here.

The history of Ceylon's party system precedes independence. The Ceylon National Congress (CNC) was formed in 1919 for the purpose of articulating the interests of non-revolutionary Ceylonese nationalists. The CNC's goals were constitutional change and its style was gradualist. While communal differences are usually de-emphasized in the *ad hoc* character of an independence movement, the moderate program of the CNC was not sufficiently compelling to submerge these differences. The CNC fragmented along communal lines. This fragmentation was the beginning and indicated the direction of Ceylon's party system.

Parties contested the 1931 election for seats on the State Council but its structure mitigated against the growth of political parties. However, independence was the real stimulus to parties and, according to Professor Woodward, from independence to the present, change is the outstanding characteristic of Ceylon's party system.

Immediately after independence, the political parties were organizations of notables. Parties offered little basis for popular identification other than the notables they could attract to run on their tickets. Because the parties had so little to offer potential candidates, there were a great many independent candidates and there was very little party discipline. The election of 1947 was contested mainly on the basis of personality. Identification with a major party apparently did not seem necessary as over half of the candidates (some 180) ran either as independents or as representatives of minor parties. None of the major parties returned over half of their candidates. The United National Party (UNP) which had hoped for a majority, nevertheless, earned a popular vote plurality. Because of this and its organizational strength, the UNP became the center of the coalition government. The UNP formed a government of comprehensive character. Unfortunately, the consensus achieved under the UNP was vague at best and never included Tamil or Marxist parties. Professor Woodward notes that from the perspective of the UNP, the great danger was that the Opposition seemed more revolutionary than loyal.

From 1947 to 1952, the parties underwent a period of self-definition and organizational development. The 1952 election was more party oriented than any previous one. The parties contested the election of 1952 more as organized units than previously. The UNP won a majority and was able to govern alone. Professor Woodward attributes the UNP victory to a fragmented Opposition, among other things.

After 1952, it was clear that the Opposition had to forge some type of unity if they were to replace the UNP as the government. At the same time, the UNP social base was eroding through neglect and this was to be the strength of the Opposition. S. W. R. D. Bandaranaike, a former UNP minister and founder of the Sri Lanka Freedom Party (SLFP), a Sinhalese socialist party, combined his party with Philip Gunawardena's Viplavakari Lanka Sama Samaja Party (VLSSP), a Marxist party, the Bhasa Peramuna, a communal organization, and a group of independents to form the Peoples' United Front (MEP). This electoral coalition capitalized on the UNP's social neglect and became the government.

The MEP could not maintain its electoral unity and by the 1960 elections, it was in disarray. This situation was a revival of the 1947 and 1952 elections where the Left was fragmented. Although the results of the election were indecisive, forcing another election, the UNP had made a comeback. In the second 1960 election, the SLFP emerged as the majority party.

At this point, it was apparent that Ceylon's party system was a viable one. The parties were capable of providing alternative governments, power was transferred in an orderly manner, oppositions learned to work together and after the 1965 election which the UNP won, the system had evidenced capacity for parties' comebacks.

Analysis of the events outlined above leads Professor Woodward to a strong admiration for Ceylon's party system's adaptive capabilities. He characterizes the system as multiparty and bipolar.

Professor Woodward's functional view of parties is the basis for his Replacement Theory of Party Growth. The Replacement Theory holds that parties are first formed as notables' vehicles to public office and the political ambitions of the notables are the main impetus to party growth. The growth of a party may be measured by the extent to which it is able to replace notables as the viable political unit.

Because the theory is inductively derived, it may have no application beyond Ceylon. More importantly, it is not fully adequate to explain Ceylonese party politics for at least two reasons.

First, it does not deal with charisma. There does not seem room under the rubric of the theory for the emergence of a charismatic leader in an established party system. Indeed, the teleology of the Replacement Theory would seem to forbid a charismatic leader. Second, the concern with party growth as a function of notables' ambitions merely implies the importance of social groups' demands. To emphasize the ambitions of notables at the expense of social demands is to deny the influence of the broader social system on the subsystem studied.

The utility of the Replacement Theory is as an ordering device, which adds a new perspective to our understanding of Ceylon's party system. The strength of the book is the description, based upon extensive research, of Ceylonese political parties and their interactions. This is the task Professor Woodward has set for himself and he has done it. —Kenneth H. Esch, *University of Hawaii.*

Army Officers in Arab Politics and Society. By Eleizer Be'eri. (New York: Frederick A. Praeger, 1970. Pp. 51. $9.50.)

Of the important Arab States of today, Egypt, Syria, Sudan and Iraq have "military regimes." The remainder—Morocco, Tunisia, Lebanon, Jordan and Saudi Arabia continue to be governed by civilians; but only Tunisia and Saudi Arabia are governed without overt support from the military establishments. Tunisia's successful reliance on its One Party, and Saudi Arabia's reliance on a fragmented tribal system have assured both states a tranquility denied the rest of the Arab states. To varying extents, the Army plays critical roles in upholding the regimes in Lebanon, Jordan and Morocco. It may be conjectured that within the very near future these too are likely to follow the path charted by other Arab regimes and succumb to the military. Should Tunisia fail to weather the test of succession to the Presidency once its ailing President removes himself from the political arena and Saudi Arabia is freed from the firm grip of its chief balancer—King Faisal—the odds are that they both will be "ripe" for the intervention of the military in politics.

To suggest that there is a certain degree of ripeness in an Arab society that makes intervention by the military likely is to run counter to one and perhaps the major thesis advanced by Mr. Be'eri, namely, that developmental requirements have little to do with bringing about military coups. His work in large measure is devoted to refuting a thesis long nurtured by Arab militarists and faithfully echoed by Western scholars that, given the social and economic conditions of the contemporary Arab world, the incompetence of the civilian systems, and the disillusionment with the limited practice of constitutional government, eventual seizure of power by the military establishment is inevitable. Mr. Be'eri quotes former President Abbud of the Sudan who upon seizing power in 1958, claimed that his coup was "natural" because the evolution of society requires at the helm a committed and competent group with sufficient discipline and cohesion to translate to reality the vision long held by the advanced ranks of the intelligentsia and radicals of a better Arab society. In one form or another, all previous writers on the intervention of the military in Arab politics have accepted the validity of this hypothesis and have maintained that the peculiar character, training and social and economic origins of the military officers make them highly suitable agents for disciplined social and economic change.

Not only does Mr. Be'eri subject this thesis to serious examination but his examination ranges widely and minutely over the course of recent Arab political history; he seeks to prove that the intervention of the military long predated radicalisation of Arab politics, or any concern and commitment to constructive social and political change. Thirty-one chapters, in six parts, to which are added two appendices, ought to provide the reader not only with a history of the successful as well as unsuccessful military coups but also with useful information on Arab politics in general and some comparative material on the intervention of the military in Latin America and other areas. The careful and patient reader will come out with a detailed if confused picture of the rationale of the military intervention in Arab politics; whether or not he is also persuaded that the intervention is primarily motivated by personal struggles for power rather than by concrete social and economic forces, he will be grateful for the abundance of the information provided. The reader will also learn much about the politics of the Arab states as these are perceived by a Marxist-Zionist scholar.

Mr. Be'eri, for example, examines minutely the achievements of the Free Officers in Egypt in terms of the liberation of the country from foreign control, industrialization, agrarian reform, etc. The reader is given enough information on other military regimes in Syria or Iraq to compare their achievements with those of the Free Officers in Egypt. Whether he will conclude, as Mr. Be'eri did, that their achievements are not as significant as their proponents claim is another matter. But Mr. Be'eri considers the limited achievements of the military regimes as one more reason for questioning the "natural" course theory of Arab politics. One of the principal rationales of military intervention is that the military can and did do more for social and economic change than previous civilian regimes. When Mr. Be'eri measures them against their own claims, the military regimes are found wanting.

The reader is left with two principal suggestions to ponder. On the one hand Mr. Be'eri's thesis is that the Arab world has had its experience with militarism—and a not very good one at that—largely because of its Islamic heritage. It seems that, since all other "explanations" of the intervention of the military in politics have been disproven empirically, then the most obvious one must be the cultural explanation. Previous writers have been equally unconvincing in suggesting this as the explanation. And second, the intervention of the military has essentially succeeded in ushering in or continuing a tendency towards fascist-like systems which, according to Mr. Be'eri, more accurately characterize Arab regimes. The left-wing rhetoric of the regimes of the Ba'th, or for that matter of Arab socialism, merely disguises fascist-like systems. Of course this too is open to serious question.

Mr. Be'eri disarms his readers at the beginning by admitting that, as an Israeli, he might not be able to write on Arab politics objectively; he also questions whether objectivity in such cases is possible or feasible. One does not have to argue the case on theoretical grounds. It is obvious that the Arab-Israeli chasm has deprived Israeli writers on Arab politics of the opportunity to appreciate the true dimensions of change in Arab society and institutions that in large measure have been effected by the military regimes. Behind the facade of authoritarian control of the center, more grass roots institutional change and participation in politics have occurred over the past two decades than in the previous century. If there is a ferment today in the Arab mind and among the intellectuals, such a ferment is in part to be attributed to the achievements of the military systems in terms of altering the social and economic structures of Arab society. That classes have not been broken and that an actual revolution by the peasants and workers has not taken place may be a question of time only. When they do occur, their appearance will have been substantially facilitated by the advent to power of the military officers. No amount of interviewing of prisoners of war, irrespective of rank, or reading of documentary accounts by an intelligence service, are adequate substitutes for a true knowledge of the complex processes of change that have been taking place in the Arab world. The failure of Israeli intelligence to assess properly the reaction of the Arab people to the June 1967 war alerts us to the fact that their inadequate understanding of the social and political bases of Arab society contrast very sharply with their correct appreciation of the military-technical situation. To rely on their documentation as Mr. Be'eri does leads to strange and in many ways obsessed conclusions.—IBRAHIM ABU-LUGHOD, *Northwestern University*.

Toward 'Uhuru' in Tanzania: The Politics of Participation. BY G. ANDREW MAGUIRE. (New York: Cambridge University Press, 1969. Pp. 393. $13.50.)

This book treats the men and events which shaped the coming of independence in Sukumaland, a tribal region in northwestern Tanzania. It covers the period from 1945 to 1965; years which encompass profound changes in Tanzanian politics. A unique feature of this study is its analysis of this change from the viewpoint of the Sukuma people, the single largest tribal grouping in Tanzania. The two decades considered here include the heyday of British efforts to direct development in Sukumaland, the emergence of indigenous resistance to these efforts, the achievement of independence, and, finally, the transition from an anti-colonial movement to a party responsible for governing the nation.

In his treatment of the first decade of political ferment in Sukumaland, Maguire analyzes the alliance which develops between African elites in the towns and discontented farmers in the rural areas and documents the importance of the rural areas in the move toward national independence. The author's treatment of British plans for promoting rural development in Sukumaland stresses the influence of individual colonial officers in the direction of local development plans and suggests the impracticality of many of these projects. In reaction to these efforts, often involving compulsion, the peasants became supporters of the urban African politicians. Significantly, Maguire shatters the neat, but frayed, traditional-modern formula for studying political change in the developing areas by showing that the anti-colonial politicians included individuals from all ranks of Sukuma society and that traditional political institutions had virtually no influence or impact on the nationalist movement. He documents the conflict between the British-maintained native authorities and the aspiring elites and shows that the emergence of the native authority system under British tutelage was in itself a violation of the traditional role of the chief in Sukuma society. Its untenable position between British authority and African society ultimately led to its emasculation before the drive of the new politicians for democratic self-government.

A detailed analysis of the development of the cooperative movement demonstrates the ties between town and village in the emergence of anti-colonial sentiment in Sukumaland. Of particular interest is the author's conclusion, conflicting with earlier studies, that the cooperative movement played no appreciable political role in the drive to independence. Although there was considerable overlap of interests, membership, and leaders between the social and political organizations and the cooperative movement, the latter limited itself, by mutual agreement, exclusively to the problems of cooperative development. This was especially the case after political activities were banned in late 1954.

Maguire shows that nationalist politics in Sukumaland predated the party of independence, the Tanganyika African National Union (TANU), by at least ten years, and traces in detail the rise, decline and transition of TANU's predecessors including the Sukuma Union and the Tanganyika African Association. He includes a particularly effective treatment of the trauma of TANU's transition from anti-colonial movement to the governing party as perceived from the villages of Sukumaland. After documenting the disaffection of local people with the often callous methods of local party officials, the author shows the effort of national officials to deal with this dissatisfaction by encouraging competition within the single-party system. The effectiveness of this effort in Sukumaland is demonstrated by the fact that prominent local TANU officials were defeated in the 1965 election and not one of the area's seven incumbent members of the national legislature was returned, including the important Sukuma cooperative leader, Government Minister and TANU stalwart, Paul Bomani.

Maguire skillfully intermingles data from official records and interviews to create colorful portraits of personalities involved in the move toward Uhuru in Sukumaland. This technique for interpreting events influenced by both expatriate officer and African leader puts individuals—particularly the often unseen middle and local level official—back into the study of political change and shows how the personalities of these officials often became the most important factor determining policy directions. A survey shows sixty-three such sketches in the book; twenty-five of which are quite detailed. Of these twenty-five, sixteen are clearly middle level and local, rather than national. Of these sixteen, five were TANU opponents during this period. The study thus also considers those neglected individuals who, though figuring prominently, do not survive in the highly competitive arena of African politics. While others may question Maguire's interpretation of all these personalities, this technique has greatly enhanced the effectiveness of the work.

Most Africanists will agree with Maguire's plea for detailed case studies as the necessary next step toward a fuller understanding of African politics. Few scholars are likely, however, to succeed as admirably as he has here. It is not that others may not possess the scholarly skills of the author; considerable as this study proves them to be. Rather the massive amount of grass-roots data upon which the study is based—constituting one of the most impressive features of the book—is due in some degree to good fortune. Considering the atmosphere in which the social scientist must today work in much of Africa and the simple passage of the first stages of independence and development, it is unlikely that future scholars will be so successful in securing information, access to participating individuals and in experiencing vicariously the transition from colonial rule to independence which was the opportunity of the author. The finished product is a credit to the officials who facilitated this research and to the author who has so skillfully prepared this study.

Two mild criticisms may be made. The Local Government Service Commission was to begin appointing Divisional Executive Officers in early 1969, not 1965 as noted in the book, and the author fails to note that all of Julius Nyerere's major writings, treated in some detail here, are available in collections considerably more accessible than the sources cited in the bibliography. These are, however, minor quibbles to a book which is a must for students of African politics and political change.—Clyde R. Ingle, SUNY College of Arts and Science, Geneseo.

Partier og erhverv: Studier i partiorganisation og byerhvervenes politiske aktivitet 1880–1913. By Vagn Dybdahl. (Aarhus, Denmark: Erhvervsarkivet, Universitetsforlaget i Aarhus, 1969. Vol. I, Pp. 406; Vol. II, Pp. 235. 90 D. Kr.)

The emergence of political science as an autonomous and vital academic discipline has followed an uneven pattern of development in Scandinavia. For the most part, Denmark has trailed far behind Sweden, Finland, and Norway in the production of systematic empirically-based political studies. Happily, the gap now appears to be narrowing. In 1959, an Institute of Political Science was established at Aarhus University. Already, several volumes which draw on the Institute's growing data bank have been issued, with several others in various stages of preparation. Vagn Dybdahl's book, while not a product of the Institute, deserves to be considered as a suitable and worthy companion piece to these publications. Indeed, Dybdahl's work, which is in reality a pre-defense printing of his doctoral dissertation, can easily be seen as a link between the practices of the past, when the study of governmental affairs was parceled out and submerged within the economics, law and history curricula, and the present aspirations underlying the newly liberated field of Danish political science.

The general task to which the author addresses himself is to explore the patterns of interaction between the various urban-based economic interests and the political system, with special attention given to the political behavior and influence of the merchant and artisan groupings. The perspective from which Dybdahl has chosen to view the matter is a historical one. Utilizing a scheme which conceptualizes the political parties as the vital representational links between societal interests and the centers of governmental decision-making, he restricts his investigation to the period 1880–1913, when the modern Danish party system was going through its formative and consolidative stages. Principal attention is confined to the two major "city parties," the Social Democrats and the Right or Conservative Party (*Højre*). Thus, Dybdahl's specific purpose is to examine the character and shape of the ties between the new political organizations and the various urban electorates at a time when these bonds are initially being forged.

If the study's historical thrust serves to establish its linkage with the classical mold, the manner in which Dybdahl has gone about his work certainly earns for the book the right to stand among the products of the more modern school of Danish political science. Not content to depend exclusively upon information taken from secondary sources, the author has fashioned his work from a formidable array of original data, much of it quantitative, gleaned from both public and private archives.

One illustration of this is Dybdahl's ingenious use of public election lists. These rolls, which contained the names of all enfranchised males together with their occupations, were frequently used for purposes of recording and tabulating individual voting choices in the period prior to the introduction of the secret ballot in 1901. Unfortunately, adequate lists are preserved only for the Copenhagen and Aarhus areas and then only for scattered elections and constituencies—ten lists in all. To his credit, Dybdahl is quick to recognize and warn of the limitations inherent in employing such scanty evidence. From this fragmentary material, however, he is able to construct for all of the major urban occupational groupings a series of fascinating participatory and partisan profiles which collectively span the two decades immediately preceding the turn of the century.

Strikingly visible, for example, is the rising involvement of the working class in the nation's political life: In 1881 only fourteen percent of the eligible blue collar voters in Copenhagen's First Election District exercised their franchise, the lowest level of activism exhibited by any group. However, according to the preserved list from the Third District of Copenhagen County, by 1898 workers were voting in that constituency at a rate approaching eight in ten—78.8%—far ahead of the other occupational electorates.

Bringing aggregate data into play, Dybdahl also demonstrates the progressive urbanization of the Conservative Party under the impact of the Danish industrial revolution. Initially a party with a balance of rural and urban support, the Conservatives increasingly found their partisans concentrated in the larger cities. Thus, Dybdahl estimates that by 1913 about seventy-five percent of the party's voters were urban dwellers.

The full range of insights produced by the author's incisive mining of a veritable mountain of source material is simply too great to mention here. Suffice it to say that in addition to probing the voting behavior of the urban population, the study focuses on three other broad areas of interest:

1) An investigation is conducted into the occupational backgrounds of the candidates nominated for the *Folketing,* the popularly elected lower chamber, between 1876 and 1913. Among other things, Dybdahl uncovers a marked disparity between the occupational composition of the parties' candidate groups and that of their respective voting publics. Generally speaking, working class individuals and urban tradesmen were consistently underrepresented among Social Democratic and Conservative nominees. A similar analysis is also applied to the members of the now defunct upper house, the *Landsting,* for the period 1866–1910.

2) Dybdahl then takes a searching look at the organizational, leadership, and program development of the Conservative and Social Democratic Parties from their respective inceptions through the outbreak of the First World War. He points out that, in contrast with the patterns found in most other European contexts, the Danish Conservative Party developed an extensive network of branch organizations more or less simultaneously with its consolidation as a parliamentary party. Of interest is the fact that manual workers made up a large share of the membership in the local Conservative Associations and Clubs which flourished in the 1880's. In the 1890's, however, the number of branches and their membership began to tail off sharply as the parliamentary struggle between the Conservatives and Liberal Party cooled and, it is to be suspected, as the organizational élan of the Social Democratic Party made itself felt among the lower classes. Although both the Social Democrats and Conservatives were dependent on the urban centers for their electoral sustenance, shopkeepers, artisans and similar self-employed tradesmen were, as was the case with the candidate groups, poorly represented on the ruling councils of both parties.

3) The relatively weak and divided political position of the urban petty bourgeoisie is one of the central themes developed by the author in his final substantive chapter. The vehicle for Dybdahl's efforts to probe the actual dynamics of the policy-making process is a case study of

the attempts launched in the early 1890's to promulgate a revised Trade Act. At this time, many shopkeepers and artisans were particularly interested in limiting access to trading licenses and securing protection against private chain operations and consumer cooperatives. Yet, a draft bill introduced first in 1893 which would have tightened the existing regulations repeatedly died in committee until it was dropped from the agenda in about 1900. In part, its fate was shaped by the ambivalent position taken by the Conservatives who, torn between a positive artisan wing and a negative free trade-industrial wing, gave only moderate and uncertain support to the measure. The Social Democrats were in much the same boat. While they were desirous of wooing the small tradesman vote, their enthusiasm was counterbalanced by a wish not to alienate the urban cooperative movement for which the party had a certain ideological and in some cases organizational affinity.

In each chapter Dybdahl musters an abundance of documentary and statistical material which serves to give his analysis depth and authenticity. So rich is this lode that a supplementary volume in which the author has collected his detailed data contains nearly 130 pages of tables, charts, and the like.

Some weaknesses are detectable. An English translation of the summary chapter, which is also included in the second volume, is rather poorly done. At one point, Dybdahl appears to have misconstrued the "cross-pressure hypothesis" and thus his contention that it is not supported by his findings is a questionable one. These are minor flaws, however, which scarcely detract from the value of the work. All in all, it is an impressive addition to the small but growing library of Danish political studies and one which students of the country's governmental affairs will undoubtedly welcome and use for years to come.—KENNETH S. PEDERSEN, *San Diego State College.*

Die Deutsche Diktatur: Entstehung, Struktur, Folgen des National-Sozialismus. BY KARL DIETRICH BRACHER. (Cologne: Kiepenheuer and Witsch, 1969. Pp. 580. $10.00.)

Professor Bracher of the University of Bonn has written a study of political mobilization, which should interest students of general comparative politics as well as modern European history. The twin engines of propaganda and organization provided the driving force of the National Socialist movement, and their study rightly serves as the unifying theme of this important book. Although the treatment of ideology both as a system of values and as a basis for mobilization is vital, the study appears to be lacking in balance, for ideology receives relatively little attention. The author describes National Socialist beliefs in the traditional manner by stressing that they were a potpourri of wholly negative appeals to anti-semitism, anti-parliamentarianism, and unrestrained nationalism. This ideological conglomerate disguised a struggle for power which ultimately led to the creation of a new Byzantinism in the form of the Third Reich.

Recent scholarship suggests, however, that the dismissal of ideology as the mere handmaiden of propaganda is misleading. Hans-Jochen Gamm (*Der Braune Kult,* 1962) and Wilfried Dim (*Der Mann, der Hitler die Ideen gab,* 1968), both of whom Bracher cites, interpret National Socialist ideology as an interlocking set of values sufficiently coherent to provide a means of legitimizing for many the regime and its policies. Gamm develops the mystical significance of National Socialist rituals and symbols, such as banners. Daim presents a view of National Socialism according to which the movement represented to its adherents the third stage (the first two having been a "paradise of racial purity" followed by an "age of racial degeneration") of a global historical process which would result in the triumph of a superior race. The eschatology of National Socialism justified the "Thousand-Year Reich" as the preparatory phase of the final battle between superior and inferior beings and made plain the essentially dialectical nature of Hitler's world view. Bracher appropriately directs our attention to a quotation from a speech Hitler gave in 1922:

> There are . . . only two possibilities: either victory for the aryan side or its destruction and victory for the Jews.

Unfortunately, Bracher does not fully develop the ideological connotations of the concepts "aryan" and "Jew." The term "aryan" proved to be far broader than the word "German," and Bracher errs in suggesting that National Socialism was primarily an extreme form of German nationalism. Indeed, Hitler's goal was the elimination of the bourgeois state and its concomitant doctrine of nationalism (cf. E. Sandvoss, *Hitler und Nietzsche,* 1969). The "aryans" were to constitute an elite recruited from America, England, Scandinavia, and the Low Countries as well as Germany. And it is historically quite apparent that only a fraction of the German nation was regarded as being biologically eligible for membership in this new master race. George Stein (*The Waffen SS: Hitler's Elite Guard at War,* 1966) provides empirical evidence to the effect that "race" and "nation" were not identical in National Socialist ideology by pointing out that by 1945, German citizens were outnumbered in the regime's praetorian guard by foreigners and ethnic Germans from outside the *Reich.* There were, for example, 125,000 West Europeans alone in the Waffen SS: an important statistic when one considers that this organization was theoretically biologically superior to all other components of the movement and therefore given the task of fathering the future masters of the world. Himmler's 1940 directive ordering his followers to kidnap throughout occupied Europe children suitable for potential SS recruits leaves little doubt as to the transnational character of National Socialism, a factor which Bracher almost ignores. The "Jew," too, was a symbolic stereotype of a mythical enemy and not always a member of a specific religious community. Goering's boast that he would determine who is a Jew is representative of this attitude.

Perhaps one of the most difficult assertions of the book to accept is the presumed historical connection between the NSDAP and the NPD. Bracher correctly identifies three common characteristics of both parties: their reliance on the Protestant agrarian vote as a principal source of electoral strength, equally autocratic internal structure, and bias against the Left and any form of internationalism. Accordingly, Bracher warns that the National Democrats (NPD) could poison the democratic political life of the Federal Republic and recommends that the party be banned. In the *Land* elections of June 14, 1970, the NPD made its best showing with a total 3.3 per cent of the vote in Lower Saxony. Among those who lost their seats in the *Landtag* was the party leader, von Thadden. Under these circumstances the proposed outlawing of the NPD would be an exaggerated response to a group which has repeatedly failed to demonstrate its appeal to the West German electorate. That Bracher and other scholars rely upon legal sanctions of this type instead of expressing their faith in the common sense of the German voter in dealing with the NPD suggests a more serious questioning of German democracy than the existence of the party itself. Nevertheless, we are not so far removed from National Socialism as to be able to ignore with impunity any manifestations of its revival in a new historical context. It was undoubtedly both as a warning and an expression of hope for a future without tyranny that Bracher dedicated this work to his children.—JAMES H. WOLFE, *University of Maryland.*

Guerillas in Latin America. The Technique of the Counter-State. BY LUIS MERCIER VEGA. (New York: Frederick A. Praeger, 1969. Pp. 246. $6.50.)

Originally published in France under the title *Technique Du Contre-Etat,* Mercier Vega summarizes the guerilla movements of recent years in Latin America. Part 1, "Chemistry and Alchemy," deals with theoretical concepts, the ambiguous roles played by the Soviet Union and China, and briefly considers the personal backgrounds of the guerilla fighters. Part II, "The National Situations," cites specific events and trends in guerilla activity in Venezuela, Argentina, Colombia, Guatemala, Bolivia, Brazil, and Paraguay, in that order. The last chapter of this Part refers in over-simplified brevity, to Chile, Mexico, and Uruguay. The latter is dismissed with a single sentence. Part III, "Documents," is at the same time boring with inconsequential details (the daily shifting of camp sites, burying of supplies, positioning of patrols, and inventory of captured weapons) and revealing of unexpected divisiveness and bitterness among leftist and Communist organizations. Among the documents reprinted—usually in attenuated form—are the diary of an Argentine insurgent, a War Ministry report by the Peruvian government, a Commission on Inquiry in Venezuela, a propaganda tract celebrating an attack by Colombians on a train, the text of the Venezuelan Communist party denunciation of Fidel Castro, and passages from Ché Guevara's booklet on guerilla warfare. Much of the volume's translation into English is by Daniel Weissbort, whose talents are less literary than literal. A necessary two-page List of Abbreviations helps somewhat in following the often tortuous course of splintered and mutually hostile revolutionary parties and "liberation" groups. The effect is that one reads bits and pieces of what is a wide-spread phenomenon affecting a dozen or more countries, with a great variation of ideological and regional implications.

Mercier Vega, a Chilean citizen, comes by his interest in, and knowledge of, guerilla tactics, honestly and pragmatically. He fought in the Spanish Civil War in the 1930's, and with the Free French forces in Africa and the Middle East during the 1940's. He also has radio and press reportage experiences of guerillas in other countries.

Altogether, the figures of Ché Guevara and of Fidel Castro, while receiving frequent mention in this slim volume, are far from dominating the Latin American revolutionary Left. Both in the chapters on theoretical concepts, and in the selection of diaries and documents, Mercier Vega makes it clear that several others aspired to national and regional leadership. There has obviously been much romanticizing of their own roles by the commanders of small bands of extremists, who time after time ran afoul of the orthodox Communist party command in their own country, and failed—as did Guevara in Boliva—to attract support of either rural peasants or urban proletariat. Potentially of great interest, where new insights would have been of significance, the author's treatment of Russian and Chinese influence, is lacking in incisiveness or novelty. The Soviets feel that coexistence assures the consolidation of the socialist camp; and "there are not sufficient grounds for concluding that a pro-Chinese apparatus on a continental scale exists." The author concludes that none of the armed movements corresponded to a theoretical scheme: "the attempts at subversion were much more of a case of classical political pressure, despair or adventurism, than of the application of the theories of the counter-state."—WILLARD F. BARBER, *University of Maryland.*

German Politics and the Spiegel Affair: A Case Study of the Bonn System. BY RONALD F. BUNN. (Baton Rouge: Louisiana State University Press, 1968. Pp. 230. $7.50.)

The Spiegel Affair. BY DAVID SCHOENBAUM. (Garden City: Doubleday, 1968. Pp. 239. $4.95.)

Die Spiegel-Affäre. ED. JÜRGEN SEIFERT. (Olten and Freiburg/Br.: Walter-Verlag, 1966.) Two volumes.

Vol. I: *Die Staatsmacht und ihre Kontrolle.* BY ALFRED GROSSER AND JÜRGEN SEIFERT. (Pp. 611. DM28.)

Vol. II: *Die Reaktion der Öffentlichkeit.* BY THOMAS ELLWEIN, MANFRED LIEBEL, AND INGE NEGT. (Pp. 522. DM28.)

The Spiegel affair resulted from a spectacular *faux pas* in the shadowy area of political justice and hastened the end of the Adenauer regime. The "night and fog" aspects of the dragnet police raids against personnel and offices of the independent news magazine and the conduct of leading members of the government in responding to public protest offended the sensibilities of important sections of the West German public. The forced resignation of Defense Minister Strauss was seen as a victory for a vigilant public opinion, reinforced by a strong echo from abroad, and as salutary evidence that the Bonn parliamentary system is responsive to public demands. But the criminal investigation and the excessive police action, which were based on an ill-founded suspicion of treason deriving from a Spiegel cover story on the Bundeswehr that attacked Strauss, pointed to weaknesses inherited from the past.

The affair raised basic issues of a strategic, institutional and legal nature, which barely reached the general public and were only gradually considered and resolved. The last implications from the Spiegel affair had not yet been drawn when these books were written from the perspective of the mid-1960's.

Their combined merit lies in pinpointing the issues and establishing the pattern of responsibility for the Spiegel action, which neither the government, parliament, the courts, nor even the opposition saw fit to clarify. The findings honor the late Otto Kirchheimer and Constantine Menges, for they confirm the soundness of their widely known study of the Spiegel case in the Harbrace Casebook *Politics in Europe,* edited by Gwendolen Carter and Alan Westin.

For a comprehensive case history of the entire affair one must turn to Schoenbaum's unpretentious carefully researched book that was written for a wider audience. A trained historian who followed the affair on the spot as a free-lance reporter, Schoenbaum pays equal attention to issues and processes. Viewing the affair as "epilogue and overture at the same time," he offers insights into the personalities, institutional processes, and problems of the Bonn system as it gradually emancipated itself from the matrix of Allied tutelage and the conserving bonds of the Adenauer regime. Regrettably the publication omits most reference notes and lacks a bibliography and index.

Bunn's effort is narrower in scope and less successful. It focuses on selected phases of the political process during the six week crisis period, which commenced with the police raids against the Spiegel—at the height of the Cuban missile crisis—and ended with the reconstitution of a CDU-CSU-FDP cabinet, or the condition that Strauss be excluded and that Adenauer resign as Chancellor in less than a year.

The specialist can turn with profit to Seifert's collection of research studies, interpretative essays and source materials, which can be discussed here only in passing. In his own major research contribution, Seifert examines in a series of sharp analytical sketches the role of the public officials in the planning and execution of the police action and the subsequent covering up of the traces of responsibility. He shows that the personal complicity of Strauss and his State Secretary is much greater than was demonstrable while the affair ran its course. He also analyzes the role of the political parties in the counter-action that shook and then restored the government coalition. Grosser addresses himself to some of the basic questions raised by Seifert, moderating the latter's rather critical conclusions from the more detached vantage point of a foreign observer who judges the Bonn Republic in contrast to Weimar and modern France. A detailed chronology of the affair and a selection of key documents complete the first volume, taking up nearly two thirds of the space.

The second volume offers a thorough documentation and provocative analysis of the public reaction to the police action, the conduct of Strauss, and the government's inept and insensitive response to justified concern. Liebel contributes a systematic and comprehensive analysis of the domestic press and, more briefly, of other specialized publics—youth groups, writers and artists, university professors, the churches and trade unions—and the results of public opinion polls and the local elections in Hesse and Bavaria that occurred during and affected the crisis. Negt examines the Spiegel affair in the mirror of the foreign press. Ellwein's interpretative essay picks up two related questions posed by Liebel. Viewed as a test case for the autonomous communication media, what conclusions can be drawn about (1) their commitment to the values of liberal democracy and (2) their influence on the decision-making political organs? Ellwein answers in a more differentiating manner than Liebel who concludes that (1) the press was only prepared to protect its self-interest and (2) the SPD and FDP would not have challenged the Spiegel action on their own. Ellwein concludes that in the Spiegel case the public performed its critical function and thereby enhanced the controllability of the government in general. A sample of press commentaries and public declarations by various groups completes this volume.

The trouble with Bunn's book is that the doubts regarding its purpose and approach, that surface in his preface and introduction, are justified. It gives the appearance of a collage of research fragments —two of which had been previously published— without a central theme, let alone an adequate theoretical framework. The author admits that he has slighted what he calls the "technical-legal features" of the affair and disclaims any intention of writing its case history. Yet the book contains material that would seem irrelevant to any other purpose. Bunn claims at one point that the study was "designed primarily to examine the functioning of the West German political system as it sought to respond to and resolve the crisis produced by the affair." It attempts more and achieves less.

More than half of the book describes the affair's background and origin. The public reaction was keyed to the impassioned crusade which the Spiegel had led for over two years against Strauss with the avowed aim to block his ascent to the Chancellorship. Bunn shows that the political system did more than respond to the crisis; it helped create it. Without the intervention of the communication media and the political parties there could not have been a major political affair. On account of the high level of public awareness and the involvement of most of the primary institutions and personalities, the crisis period lends itself to a systematic analysis of the Bonn system's styles, procedures, and power differentials. Bunn attempts this only in part and hesitates to draw conclusions. Some processes are overlooked, others barely noted. For example, a full chapter exploits a previously published analysis of a limited sample of newspaper clippings for the purpose of discussing the role of the press in framing the issues and articulating the protests of other organized publics immediately after the initial police raids. The subsequent role of the communication media in intensifying the crisis is virtually ignored. Liebel demonstrates that a large segment of the press abandoned its initial reserve only after Strauss' previously denied involvement had been partially unveiled at the end of a series of parliamentary question hours.

Of all the actors, only the FDP receives the benefit of sustained analysis. For Bunn seems convinced that it alone was in a position to resolve the crisis of confidence by enforcing the principle of ministerial responsibility against Strauss through its temporary withdrawal from the government. He clearly shows the ambivalence of the FDP leadership, then headed by Mende, as it was driven by the pressure of events to play its promised role as the liberal corrective against the abuse of power in a bourgeois coalition. One might wish that Bunn had paid even more attention to the internal processes and the parliamentary role of the FDP, to the exclusion of other concerns, for it is here that the book contributes to our understanding of West German politics.

Bunn cautiously suggests that the FDP's "third force rationale is not entirely irrelevant to the Bonn system." But he fails to contemplate the possible consequences, if the affair had occurred two years earlier when the CDU/CSU possessed an absolute parliamentary majority, or to discuss the implications of the trend towards a two-party system. It could be argued that the presence of a parliamentary third force merely affects the style of settling conflicts without altering a political system's capabilities.

Bunn concludes with a catalogue of institutional strengths and weaknesses, but one is left to wonder about the nature and viability of the Bonn system. He praises the control function of an independent press, the SPD opposition, and the FDP coalition partner. He notes the efficacy of the parliamentary question hour as a control instrument in exceptional situations. He also identifies two flaws which have been since corrected: (1) the automaticity of criminal prosecutions under the "legality principle" in cases of political justice; (2) the practice of assigning State Secretaries as watchdogs on Ministers of the coalition partner. But, on the whole, his presentation leaves the impression that the origin of the affair, as well as its resolution, were the accidental product of circumstance and personality.

Schoenbaum attaches greater significance to the largely autonomous role of the federal prosecutors and the judiciary. Apart from the encouragement and direction it received from Strauss and his State Secretary who presumed to act on behalf of Adenauer, the Spiegel action was the product of anachronistic law, inadequate procedures, and poor judgment on the part of a state bureaucracy that uncritically accepted the judgment of constituted authority and frowned on those who questioned it.

Ultimately the affair posed the key issue—later explored before the Federal Constitutional Court —where, and by whom, the line is to be drawn in concrete cases that might involve a confrontation between the right to inform and criticize, as a correlate of public control of military policy, and the duty to protect secrecy, as a correlate of national security.

Schoenbaum explores the origin and the implications of the incriminating cover story that attacked Strauss' strategic conception, divulged the Bundeswehr's inadequate performance in the recent Fallex-62 NATO maneuvers, attributed this to Strauss' faulty planning, and suggested that Strauss' atomic ambitions were being opposed not only by Bonn's alliance partners but from within the Defense Ministry. A full English translation of the story is appended to Bunn's book, which offers a brief summary but otherwise avoids the problem.

The purpose of the police action was to secure evidence in support of the charge that alleged state secrets in the cover story had been procured by the Spiegel from as yet unknown sources in the Defense Ministry. Military publicists in the West found nothing that jeopardized NATO security and little that had not been previously published elsewhere. A month-long search of the Spiegel offices and archives failed to uncover the vast conspiracy of treason and even high treason that Strauss and Adenauer imagined and the federal prosecutors thought possible. It merely netted a Bundeswehr colonel who had contacted the Spiegel out of concern over Strauss' preference for a pre-emptive atomic strike strategy at a time when the Kennedy administration switched to a strategy of graduated deterrence. The federal prosecutors' efforts to vindicate themselves through convictions failed to bring a single defendant to a formal trial. If trials had been held under the existing law, which derived its pedigree from the Bismarck era, they might have led to politically inopportune convictions and a miscarriage of justice. The last cases were dismissed for insufficient evidence in 1965. Three years later the law was liberalized under the guidance of Justice Minister Heinemann, formerly an attorney for the Spiegel.

All authors agree on the culpability of Strauss. Grosser allows that if Strauss had resorted to half-truths and untruths for reasons of state it would have been excusable. That he lied to the public and parliament mainly for personal advantage rendered his conduct morally objectionable and his continuance in office politically untenable. Here the parallel to the Profumo affair that rocked the Macmillan government one year later ends, for Strauss was able to recapture ministerial honors. As long as he remains the undisputed leader of the CSU, he will be a potent national figure on account of the constellation of Bonn politics. Bunn and Schoenbaum describe the bargaining reality of a parliamentary four-party system throughout the government crisis. We still lack sufficient knowledge about the internal structure of the CSU to be able to explain the phenomenon of Strauss, but it seems clear that the Bavarian electorate's failure to punish the CSU at the height of the Spiegel crisis actually strengthened his leadership. Since the CDU and CSU are Siamese twins with separate identities and inseparable bodies, the CSU—and consequently Strauss —were bound to exercise a disproportionate amount of influence. This could not be changed as long as a SPD-FDP governing coalition was impossible or impracticable for arithmetical and attitudinal reasons.

Taking the long view, Schoenbaum suggests that the Spiegel affair, not unlike the Dreyfus affair in the Third Republic, may be seen as a turning point in the evolution of the Bonn Republic. From the perspective of the mid-1960's—as Strauss was returned to the cabinet under the aegis of a great coalition which in the eyes of many intellectuals signalized the atrophy of responsible government and the malaise of social stagnation—most of the other authors share the then fashionable view that the Spiegel affair temporarily disturbed the system without changing its premises. They underrate the affair's demonstration effect tied to rapid generational change—in our post-industrial society a question of as little as five or ten years. Their own accounts show that the affair brought out stylistic predispositions and substantive concerns that challenged established patterns. The government's authoritarian demeanor as well as the efficacy of political protest had a profound effect on several attentive publics, especially among the young whose political consciousness was formed under the Bonn System. The methodological problems standing in the way of measuring demonstration effects need not keep one from postulating more or less direct connections between the Spiegel affair and each of the following: the rise of an extra-parliamentary opposition, the modification of the proposed state of emergency legislation, the liberalization of the criminal code and judicial practice in the whole area of political justice, the formation of a social-liberal SPD-FDP coalition, and major changes in Bonn's foreign and military policy along lines propagated by the Spiegel for more than a decade.—CHARLES R. NAEF, *Colgate University*.

INTERNATIONAL POLITICS, LAW, AND ORGANIZATION

The American Threat: The Fear of War As an Instrument of Foreign Policy. By James L. Payne. (Chicago: Markham Publishing Company, 1970. Pp. 241. $7.95.)

This book purports to broaden understanding of international affairs through an analysis of the threat of war as an instrument of foreign policy. Explicitly eschewing any intent to advance specific policy proposals, the author pleads a larger purpose, namely, "to delineate some basic problems and perspectives of the statesman, particularly of the American statesman in the Cold War era." (p. xiv)

For a number of reasons the study falls conspicuously short of its mark. Conceptual difficulties arise early when the author confuses his explanation of state behavior, through what is offered as a theory of threat, with his defense of the use of coercive threats as the principal justification for American policy abroad. In attempting to construct a behavioral foundation for his analysis, moral and legal principles are rejected as satisfactory explanations of state action. According to the author, the superiority of one moral code over another cannot be validated; and legal rationales can buttress any claim, however absurd. The author's kind of statesman acts, therefore, in terms of his understanding of the status quo defined as "the mutually perceptible distribution of rights over which war may be provoked between hostile nations." (p. 66) The author would have us accept "right" to mean an enemy state's perception of the physical punishment it would incur if it violated the "rights" of its opponent. In this view, aggression becomes the transgression of an opponent's right as the opponent views it and as that right is perceived by the other rival. The congruence of the two views defines the status quo. Under conditions of Cold War, where the United States is pictured as engaged in a worldwide struggle against implacable foes, global peace is said to hinge on the articulation and consistent defense of a status quo in which "ours" (American) and "theirs" (Communist) is clearly defined. (p. 76)

Defining the status quo offers no insurmountable difficulties for the author. It is tantamount to the categorical imperative that the United States assume global defense commitments everywhere in order to meet its pledge to oppose forceful Communist expansion anywhere. This obligation is allegedly thrust upon the United States more as a required response to Communist expectations of American behavior than as a moral or political duty. Even small incursions by lesser Communist states must be met by a coercive response lest the credibility of the American deterrent be weakened and larger Communist powers, like Russia and China, be encouraged to embark on more ambitious imperialist ventures.

A number of questionable devices are employed to demonstrate that the author's logical analysis is father to contemporary state behavior. At various points American strategic problems are reduced to simplified allegories or analogies. The United States is compared to a lonely traveler pursued by a wolf pack (the Communist states). He must attack even the harmless cubs otherwise the others "will suppose that the prey is weakening and may close in." (p. 112) Or, the complex choices confronting American strategists are reduced beyond recognition to a scenario in which "Lightening Joe," the aspirant sheriff, must draw on "Rough Pete," the town gunman, if expectations have been raised, even through groundless rumor, that he has made a challenge. (pp. 123–24)

Motives are freely attributed to world leaders to illustrate their presumably compulsive concern with the maintenance of a credible deterrent posture over all other state objectives. When former Secretary of State Dean Rusk is quoted as saying that the United States has security commitments with just over 40 states, the author gratuitously allows that Rusk really meant to say the entire non-Communist world, but was constrained from being more explicit out of deference to domestic criticism. (pp. 128–29) President Kennedy's supposedly flaccid management of the American threat establishment is held responsible (causally and morally) for Khrushchev's precipitation of the Berlin Crisis in 1961–62. Explanations of preceding Berlin confrontations go begging. Similarly not addressed is the significance for deterrence theory of Russian responses to the largest military demobilization and one of the largest mobilizations of military force in American history, respectively, under the Truman and Kennedy administrations. Even de Gaulle's decision to build a *force de frappe* is laid to Kennedy's hesitancy to employ force despite the troubling fact that the inauguration of the French strategic strike force preceded his entry into office.

On the other hand, the Johnson administration's expansion of the Vietnam War in 1965 is alleged to have frustrated Chinese attempts to launch " 'a tri-continental people's war,' " (p. 148) and its dispatch of troops to Santo Domingo is said to have strengthened the American global deterrent which, in the author's mind, was crucial to avoid "a long and costly struggle" in Vietnam. (p. 205)

Except for random citations from *The Pen-*

kovskiy Papers, the reader looks largely in vain for original documentation, however inconclusive, to sustain the author's interpretations of Russian, Chinese, and American behavior. But in the absence of such evidence, how can the principle of mutual perceptions of status quo rights be applied? Moreover, it is precisely in those areas where the great powers are in dispute that the status quo is mutually in question. The author's conception of the status quo loses its operational relevance at the very moment when the demand for clarity of rights between rivals is most pressing. Nor can the author's notion of the status quo be saved by illogical leaps that oblige the United States to extend uniform and unreserved security guarantees to all non-Communist countries and to respond to all forms of Communist aggression automatically and inexorably. (p. 138) This antiseptically presented imperative loses much of its force when it is revealed as a thinly disguised policy recommendation resting on scanty and selective historical data and on perhaps arresting, but imprecise, comparisons. And, as a matter of record, American presidents have been more cautious and flexible than the author's procrustean logic admits in minimizing American security guarantees to non-Communist states. Even its formal treaty commitments have been differentially defined, depending on the ally, in the NATO, SEATO, RIO, and ANZUS Pacts and in bilateral accords with South Korea, Nationalist China, and Japan.

The author's attempts to base American military strategy on depoliticized principles of action falsifies the problems facing American statesmen. The study dismisses public opinion as "schizophrenic and animalistic." (p. 194) A democratic people are portrayed as incapable of appreciating the necessity of projecting threats abroad. Yet how can such threats be effectively marshalled if the human and material requirements for their construction cannot be raised among the populace except in appeals to political interest and prudence, legal rights, and moral duty—all of which are repudiated by the author as insufficient bases for state action?

The critical problem facing the American statesman is to establish a viable equilibrium between competing requirements for domestic consensus and national security. Offering for emulation the examples of such imperialists as Frederick the Great (pp. 12–13, 154) and Disraeli (pp. 196–200) deflects our attention without improving our understanding of the problems facing American statecraft today. Doubts may be registered, too, over the author's plea for more faith in those American statesmen who apply "tradition, precedent, and received knowledge" to contemporary problems. (p. 195) Whose tradition should be followed? Alexander Hamilton or Thomas Jefferson? Theodore Roosevelt or Woodrow Wilson? Skepticism may be also raised over the author's call for more trust in "middle-echelon bureaucrats" whose enobling task is "to check proposals to see that they are consistent with that great bundle of tradition and value which forms the only secure locus for a safe and, ultimately, a moral foreign policy." (p. 201) A democratic people may be ably served by its bureaucracy, but it cannot survive the abdication of its political responsibilities to public servants who are exempt from an electoral test.

What begins as an attempt to present a behavioral understanding of the use of threats in American foreign policy ends with the soporiphic intonement of pastoral counsels of doubtful strategic efficacy, political applicability, or moral validity. Judged by the author's own standards, the conclusion is difficult to resist that this study confuses, more than clarifies, "the problems and perspectives of the . . . American statesman in the Cold War era."—EDWARD A KOLODZIEJ, *University of Virginia.*

The Great Powers & Africa. BY WALDEMAR A. NIELSON. (New York: Frederick A. Praeger, 1969. Pp. 404. $11.95.)

For the Africanist or student of American Foreign Policy it will be most profitable to begin with the final section of this book which suggests a future U.S. policy toward Africa. The general reader should read the previous chapter for background information on the post-colonial African policies of Britain, France, Russia, China, Belgium, Germany, and the United States. The comparison of American Foreign Policies with other countries active in Africa is useful for pointing out the dimensions of our policy—in this case the shallowness.

Nielson, President of the African-American Institute, strongly argues for a revision of U.S. policy. As he sees American policy, it has deteriorated from a barely adequate framework under Kennedy to a clearly inadequate one. Even more gloom is ahead, he fears, because of the general reaction against any active American policy toward developing countries. The reaction to the Vietnam War may account for the general unwillingness to formulate policies which would call for continuing or increasing commitments of money, staff, and planning. However, Africa has always had a low priority in American policy, and as we have gained confidence in understanding African developments, we have once again returned Africa to the bottom of the list.

The need for a bold policy is derived from several sources: first, a growing constituency for Africa among the blacks and the youth in America; second, the national interest in preventing great power confrontation by insuring progress among African nations; third, the nations vision of itself and its relations with others as the "dominant civilization in the world of the late twentieth century."

In making the case for an active American policy toward Africa the author defines four issues as central to American interest. (1) What will be the future relationships among African and European countries, especially former colonies and the metropoles? (2) What is the American interest in fostering political change in the "White Redoubt" of Southern Africa? (3) What should be American strategic and military policy in Africa? (4) What should be done about problems of development? These four general areas are certainly central to U.S. policy, and the author has posed the problems in their toughest form in order to make clear his argument for an active U.S. policy.

The trend in American policy which has led to pessimism in Africa regarding our willingness to provide capital and technical assistance should be reversed. Reducing aid and limiting bilateral agreements dissipated the best policy tool available in Africa, and the U.S. has set a model for European countries to limit their aid appropriations. Moreover, the U.S. has not maintained leadership in making funds available through agreements and international institutions. In 1962 the U.S. contributed .8% of GNP in public *and* private aid; in 1967 it was down to .57%.

He also calls for a stronger stand against the minority governments in Southern Africa. This policy would not aim at overthrowing governments by force. There are good reasons for not committing the U.S. to military endeavors in Southern Africa, as most readers are likely to agree. However, short of military assistance there are a number of ways for the U.S. to help bring about change. An approach of *creative tension* would search for the economic and political actions which would develop the nationalist forces in Southern Africa as a "driving, courageous, unified, sustained, and intelligent" movement.

The U.S. should continue to define its responsibilities for peace-keeping and international security in a narrow way. However, the U.S. should not assume that a "low profile" will prevent problems from developing in a way which will eventually require our unilateral reinvolvement. Rather, the U.S. should try to strengthen the multilateral forces, particularly the OAU, to deal with African situations without recourse to great powers outside Africa. The U.S. should encourage tacit agreement with other great powers to abstain from intervening in African affairs. In a "strategically sterile" environment the U.S. would find few reasons to be drawn into local affairs. The danger af intervention for strategic (anticommunist) reasons was illustrated in the Congo. Although the U.S. generally did not become a competitor of the USSR in the Nigerian war, a whole series of conflicts which would tend to involve the U.S. can be imagined easily.

The fourth policy change would encourage the U.S. to work *with* European countries in aiding Africa rather than leaving the problem to them. American action and initiative is crucial to developing an over-all framework for trade and capital. This problem has wider implication than just Africa, for restructuring U.S.-European relations also requires American initiative. Africa only becomes part af an unassembled triangle.

Nielsen's arguments attempt to define positively the American interests in Africa. It is a long-range policy, set by an image of ourselves which emphasizes the best of American international relations. This does not permit a minimal involvement.

By defining the *goals* we ought to pursue and the policies which are likely to further these goals, the debate about American policy is likely to remain rational. As long as we formulate conceptions of American foreign policy with the understanding of internal and international developments, that Nielsen has shown, there is hope that American power may be an effective force for development. Unless we make these kinds of assessments we are in danger of forming policy on the basis of short run "budgetary constraints," or "isolation sentiment," which do not help us decide how power should be used.—JAMES E. LOESEL, *Washington and Lee University*.

Yugoslavia and the Nonaligned World. BY ALVIN Z. RUBINSTEIN. (Princeton: Princeton University Press, 1970. Pp. 353. $11.00.)

Notwithstanding the fact that an immense amount has been written on the subject of Yugoslav foreign policy, very little attention has been payed to the country's relations with the newly emerging nations of Africa and Asia. Yugoslavia has nevertheless played a key role in efforts to form a non-aligned bloc, and close relations with the developing nations have been a primary concern of Yugoslav foreign policy in recent years.

Professor Rubenstein, with the skill and insight born of a thorough knowledge of Communist diplomacy, has chronicled Yugoslav efforts to create a coalition of the non-aligned nations, a group which could be an active factor in strengthening the United Nations, hastening the dissolution of military blocs, and—in the spirit of the UNCTAD conference of 1964—developing greater trade between the developed and lesser developed nations. The resulting volume provides important insights into the evolution of Yugoslav foreign policy as well as shedding new light on the problems that have accompanied the development of the concept of non-alignment.

Yugoslavia, in the author's view, initiated the campaign to create a non-aligned group of nations out of practical considerations. The desire of the Yugoslavs to avoid diplomatic isolation, hopes of economic gain, and the fact that stressing ties with the newly-emerging nations was a policy calculated to provoke a minimum of dispute within the Yugoslav Party, all were important considerations guiding Yugoslav actions. At the same time, one of the most interesting and valuable aspects of Professor Rubenstein's treatment concerns the underlying principles which have guided the Yugoslavs in developing the idea of non-alignment.

For, as this account suggests, Yugoslavia differs from the other non-aligned and newly emerging states not only by virtue of the fact that she is a European power, under Communist rule, and only partially underdeveloped, but also because she is more genuinely internationalist and more adversely affected by cold war tensions than her African and Asian partners in non-alignment, many of whom have become primarily "regionalists" in recent years and (as in the case of Nasser) not adverse to accepting the economic and military aid which flows from great power rivalries. Although siding with the radical nationalist leaders of African and Asia on most issues, Yugoslavia's approach toward non-alignment has been more consistent, and in the last analysis more constructive, than that of most of the third world nations.

The author's conclusions nevertheless raise the question of whether Yugoslavia will continue to place as much emphasis on relations with the African and Asian nations in the future as in the past. Regionalist tendencies have been driving the newly emerging nations away from the broad principles of non-alignment congenial to the Yugoslavs. At the same time Yugoslavia faces pressures to concentrate more on economic and diplomatic problems arising from the integration of Western Europe and the renewed threat posed by Soviet military activities in Eastern Europe and the Mediterranean. Personal diplomacy, in which Tito has placed such great store in his relations with the third world, has suffered a setback with the death or removal from power of the radical national leaders of the 1960's. Finally, the growing domination of the Soviet Union over Egypt has been a source of concern to Yugoslavia and has raised certain doubts concerning the utility of Yugoslavia's unswerving support of Nasser and his policies.

While examining basic questions connected with the evolution of Yugoslavia's policies toward the non-aligned nations, Professor Rubenstein has also sought to show how the multifarious instruments of modern diplomacy—work in international organizations, offers of economic assistance, or the development of personal contacts among leaders, for example—have been utilized by the Yugoslavs in their campaign to make themselves the leader and spokesman for the non-aligned group. Although the result is a certain amount of repetition (the book is organized more functionally than chronologically), this approach does provide insights into the techniques employed by a medium-sized power with limited resources in pursuit of rather ambitious foreign policy goals.

The author is lavish in his praise of the skill of Yugoslav diplomats. He does nevertheless point out what appear to be inconsistencies in the Yugoslav approach to peaceful co-existence and non-alignment (for example, the Yugoslav failure to react to the intervention of Egypt in the affairs of Yemen). He is quite critical of the Yugoslav position at the Belgrade conference of the non-aligned in 1961; one would like to hear the Yugoslav viewpoint on this famous incident in which Tito downplayed the importance of resumption of nuclear testing by the Soviet Union. On the other hand there are parts of the book in which the official Yugoslav point of view is perhaps overly stressed, if only because the Yugoslavs were very free in providing the author with information on the Yugoslav position on the problems under discussion.

Professor Rubenstein's account is a skilled and mature analysis of a complex subject important both to students of the problem of non-alignment and persons interested in Yugoslavia. The book should contribute to a new, and broader, approach to Yugoslav foreign policy, which up to now has been considered almost exclusively from the point of view of cold-war politics.—Paul Shoup, *University of Virginia.*

Arms For The Third World: Soviet Military Aid Diplomacy. By Wynfred Joshua and Stephen P. Gibert. (Baltimore: The Johns Hopkins Press, 1969. Pp. 169. $6.95.)

As the authors of this compactly-written little book demonstrate, military assistance has become an essential instrument of Soviet foreign policy toward the underdeveloped world. Since the 1955 arms agreement with Egypt, Moscow has distributed some $6 billion in arms to 25 countries in the Third World, an amount slightly larger than that spent for the same purposes by the United States.

The authors find three major political objectives underlying the Soviet arms aid program. First has been the Kremlin's desire to weaken Western, especially American, influence particularly in countries close to the Soviet border. Thus, Soviet aid in the 1950's to Afghanistan, Egypt, Syria and India was, at least in part, designed to undercut John Foster Dulles' efforts to join Turkey, Iraq and Pakistan in a defensive alliance aimed at the Soviet Union. A second major objective has been an offensive one, i.e., to extend Soviet political influence in the developing world. By means of arms aid to countries like India, Ghana, Indonesia and other eminent representatives of the Third World, Moscow seeks to promote the image of the Soviet Union as the champion of anti-colonialism and national independence and to foster military and, hopefully, diplomatic dependency on the USSR. A third aim, one whose importance has grown significantly during the past decade, relates to Moscow's desire to counter the growing activity of Communist China. Russian military aid policies in South and Southeast Asia and, to a lesser extent, Africa, seek to prevent China from becoming the predominant source of revolutionary leadership in the Third World.

In light of these three objectives, which obviously overlap and intertwine, the authors analyze Soviet military aid to the major regions of the developing world. While often sketchy, this regional survey pulls together a considerable array of hard-to-find information and, more importantly, clearly outlines the main features of Soviet military aid diplomacy. They observe, for instance, that Moscow has chosen to concentrate its arms aid program on relatively few countries. In fact, as they note, 86% of all Soviet military aid has gone to the countries of the Middle East and Asia, with Egypt, Indonesia, India and Iraq being the main recipients. Only 1% of the total Soviet aid program (as of 1967) went to Africa and the 13% which went to Latin America was all sent to one country, Cuba.

These figures, according to the authors, clearly demonstrate the essentially conservative character of Soviet arms policy. "The military aid program indicates . . . that Soviet policy, like that of imperial Russia, still emphasizes regions adjacent to the Soviet Union and is still cautious about moving into distant areas. . . ." (p. 132) While generally true, the authors themselves supply evidence which seems to contradict this interpretation. Substantial Soviet aid to distant Indonesia, which alone received $1.2 billion in arms (over 20% of the total) and to Cuba clearly indicate rather broader horizons than they suggest. Further, as the text notes, influence can be peddled rather cheaply in the underdeveloped world. Thus, for the relatively small sum of $10–15 million in military aid, Moscow managed to place 1000 advisors in far away Ghana, some of whom were assigned command positions in Nkrumah's private army. In fact, at the time of the successful coup in February 1966, a number of Russian officers were killed in the attack on Nkrumah's official residence. Thus, though generally careful in their arms aid policy, the Kremlin seems more venturesome than the authors are willing to admit.

Soviet caution is further revealed, in their judgement, by Moscow's practice of limiting the type of assistance it is prepared to offer. Thus, despite a 1965 request by Egypt, the USSR has refused to supply nuclear arms to any Third World nation. Furthermore, Moscow's aid to Cuba since the 1962 missile crisis has been limited to defensive weapons. In addition, as the authors point out, the bulk of Soviet aid to the North Vietnamese and Viet Cong forces have been mainly weapons useful in fighting a limited conflict.

While such policies, to be sure, indicate a sense of self-restraint, it is open to question whether the USSR's policies are as cautious as has been suggested. In the past, where circumstances have seemed propitious, the Soviet leaders have been strikingly bold in their arms policies. In 1962, for example, missiles with major offensive capabilities were positioned in a Cuba far removed from the Soviet border. Furthermore, from 1961 through 1965, Moscow provided the armed forces of a highly belligerent Indonesia under Sukarno with a large number of modern offensive weapons. And, more recently, Soviet military aid to Nasser directly contributed, as the authors admit, to the outbreak of the Six-Day War. Since 1967, despite the increasingly serious danger of renewed conflict and the growing risk of an American-Soviet confrontation, Soviet military aid to Egypt has steadily increased.

Rather than "conservative and cautious," (p. 126) I prefer their terms "pragmatic and opportunistic." (p. 157) This characterization would have come across more clearly had the authors referred to Moscow's earlier experiences in military aid diplomacy. During the 1920's, the Kremlin extended military aid to anti-Western movements in the underdeveloped world, including those led by Kemal Attaturk and Chiang Kai-shek, despite their hostility towards local Communist elements. In the late 1930's, Moscow provided the Kuomintang with considerable amounts of arms and ammunition, sent over 3000 advisers to China and allowed several hundred Soviet airmen to fly with Chiang's fledgling airforce. This assistance, designed to support China in its war with Japan, was also used by Chiang against the Communist forces of Mao Tse-tung. In the mid-1940's, with the aim of expelling British influence from the Middle East, the USSR furnished arms to the Israelis. Without this assistance, Israel quite conceivably would not have survived, a point which both Moscow and the Arabs would undoubtedly prefer to forget. While the analogy is somewhat strained, Soviet policy toward the war in Vietnam is curiously reminiscent of its highly pragmatic attitude toward the Spanish Civil War. Too obligated ideologically not to give any assistance, but fearful that Communist success might lead to undesirable consequences—the creation of a Communist Spanish Republic might have frightened Britain and

France into the arms of Germany and Italy while a completely Communist Vietnam may align itself with Peking—Moscow in both cases has pursued a policy which does not give major support but merely makes a show of support.

The authors of this study are to be commended for a fine general survey of recent Soviet military aid diplomacy. Their comparison of Soviet and American military aid programs, whose general features are surprisingly similar, is of considerable interest. The work is marred, however, by poor editing. More careful monitoring for inconsistencies might have reduced the contradictions between the general conclusions and the evidence cited or, at least, compelled the authors to seek to reconcile them. The book, in addition, would have benefitted from a stronger analytic perspective. Other than a rather brief attempt at correlating the UN voting records of Third World nations with those of the sources of the military assistance they receive, there was little systematic effort to analyze the effectiveness of the Soviet military aid program. Despite these shortcomings, this is a highly useful study of a little-examined and distressingly important subject.—Morton Schwartz, *University of California, Riverside.*

The Foreign Policies of France 1944–1968. By Guy de Carmoy, translated by Elaine P. Halperin. (Chicago: the University of Chicago Press, 1970. Pp. 510. $15.00.)

Guy de Carmoy has attempted to catalogue and assess the major foreign policies of the Fourth and Fifth Republics, as well as the general attitudes of the contending domestic groups. He has done so less from the point of view of a detached observer of events than a partisan. Indeed, M. de Carmoy ran in March 1967 for the National Assembly as a supporter of François Mitterand. The protagonists of European unity clearly emerge as the heroes of this interpretation of French foreign policy. And, just as clearly, Charles de Gaulle is cast in the role of villain. This is as true of the Fourth as of the Fifth Republic.

De Carmoy organizes his analysis of the policies of the two republics in terms of the "three major trends [which] have altered the realities of international politics during recent decades" (p. ix): the new hierarchy of states which emerged from war, technology, and demography; the decolonization movement; and the construction of Europe. In general, he believes that the leadership of the Fourth Republic on the one hand and General de Gaulle on the other adjusted to these trends in radically different ways. Rather than seeing a general continuity of policy from the Fourth to the Fifth Republics, De Carmoy perceives the Fourth Republic as "enclosed between the two reigns of Charles de Gaulle" (p. 174), an important and, in foreign affairs, basically sound interlude between the "vast enterprises" of General de Gaulle.

He argues that the Fourth Republic ultimately failed because it "pursued objectives that were too ambitious for her limited resources . . . [and] could not be reconciled because of the conflicting ideologies that underlay them" (p. 182). Specifically, the leaders of the Fourth Republic erred in attempting to follow a global policy based on colonial possessions while at the same time seeking continental security and reconciliation. The inability to escape from the colonial mission or to follow consistent supranational policies in Europe and the Atlantic area were the result of a heavy historical and ideological baggage and of the exigencies of domestic political pressures. In effect, the "innovative" foreign policies of the Fourth Republic in respect to European unity and atlantic alliance were constantly jeopardized by domestic divisions and colonial policy.

Foremost, in De Carmoy's view, among the domestic political pressures which undermined the "positive" thrusts in the policies of the Fourth Republic was General de Gaulle. In the three areas of defense, colonial policy, European unity, M. de Carmoy essentially portrays a morality play with the forces of Gaullism progressively in the ascendant. Hence, he concludes on the question of defense that "the idea of joint defense and loyalty to the Atlantic Alliance conflicted with the notion of an exclusively national system . . . France's Atlantic policy declined as the Fourth Republic lost ground and the Gaullist opposition gained." (p. 62). And on the subject of Europe, "divided on all major problems, [Frenchmen] had in their ranks the most eminent champions and detractors of a united Europe" (p. 124). With regard to decolonization, De Carmoy declared that "successive governments, accused by de Gaulle and his friends of abandoning French colonial interests, were unable to formulate a consistent policy or explain to the public the often painful alternatives that inescapably confronted the nation" (p. 173). The pressures, concluded De Carmoy proved to be too much and a crisis of regime brought to power "a man of the past . . . largely responsible for the inconsistencies of French foreign policy" (p. 189).

M. de Carmoy does give General de Gaulle qualified praise for improving France's domestic situation and for extricating the Republic from the Algerian tangle. Yet despite the thrust given industrialization by the monetary reform, it could not keep pace with an expanding population and accelerated urbanization. The character of de Gaulle's assault on the international monetary system and the European Communities, as well as the project for an independent deterrent, are portrayed as in part to blame for France's internal ills. De Carmoy's principal criticism in these areas seems directed at the presuppositions and manner of de Gaulle's actions which denied the ultimate validity of a supranational framework, which is seen by De Carmoy as essential to a strengthened European economy. Moreover, by treating collective frameworks as purely instrumental, de Gaulle's policies threatened as well the organic ties which linked the German Federal Republic to Western Europe.

In the area where General de Gaulle is often praised, the resolution of the Algerian conflict, M. de Carmoy is also reserved. Basically he believes that de Gaulle was too slow to launch negotiations, alternately too ambiguous and too rigid in his positions, too determined to treat the problem as exclusively French, and too inconsistent in his policy preferences. De Carmoy does not interpret these actions so much as indicative of de Gaulle's attempt to maneuver hostile and contradictory elements in the French polity, but as exacerbating those elements and eventuating in the least favorable termination of French sovereignty in North Africa.

In traditional fashion, De Carmoy interprets de Gaulle's policy objectives as independence and great power status for France. The Franco-German alliance, the Atlantic Alliance, the European Communities, the African associations, bilateral arrangements are all seen as means rather than enduring ends. M. de Carmoy concludes in fact that de Gaulle was able to limit the various instruments of Western unity without, however, improving France's security or its long-run influence. And, indeed, in one of De Carmoy's most provocative theses, de Gaulle's rapprochement with Russia is seen as endangering the entire continent. The joint Franco-Soviet declaration of 1966 is portrayed as "a step toward a reversal of alliances" (p. 480) and a "Europe from the Atlantic to the Urals" over which France and Russia would preside and through which Germany would be neutralized. In effect, "the Fifth Republic reversed the foreign policy of the Fourth; during his second reign Charles de Gaulle has pursued the foreign policy he began during his first" (p. 478). The result of this venture, however, will be, in M. de Carmoy's view, not a Franco-Soviet condominium but Soviet domination.

M. de Carmoy's thesis is well-stated but often too little documented. This is particularly true of his analysis of the Fourth Republic. His discussion of such issues as the European Defense Community, the Suez crisis, and the French decision to acquire a nuclear capacity are presented in cursory fashion. In many chapters he seems to chronicle dates and events without probing motivation and meaning. Moreover, the book would have been strengthened by a better analysis of the domestic forces and constraints in the policy process. This would have been particularly helpful in assessing the years just before and after de Gaulle's return to power.

On the other hand, several of De Carmoy's chapters are well-stated, interpretive essays. Indeed, many of his chapters on the Fifth Republic seem to fall into that category. As such they are often extremely provocative and sweeping theses.

Guy de Carmoy's study is a well-organized and written interpretation of French foreign policy since 1944. It does not constitute a thorough and balanced appraisal of that period, but it does provide a generally interesting introduction to France's post-war policy.—Robert S. Wood, *University of Virginia.*

Soviet Foreign Policy in Perspective. By Robert G. Wesson. (Homewood, Ill.: The Dorsey Press, 1969. Pp. 492. $9.50.)

To the extent that our survival depends on policy choices, the foreign policies of the major, i.e. thermonuclear, powers are crucial. Yet despite some two decades of intensive research into Soviet politics, we long lacked a comprehensive, interpretive history of the foreign policy of the Soviet Union. Perhaps in response to the semi-centennial urge, a number of surveys of Soviet foreign policy have now appeared. Of each it may be asked: Is its historical narrative reliable? Does it provide insights into Soviet foreign policy or demonstrate the application of new methodologies to the subject matter? Which is its most suitable audience?

Writing on Soviet foreign policy so as to elicit affirmative answers to such questions is a formidable task. That lack of reliable material common in studying Soviet domestic politics is only partly offset by the records of states which have maintained foreign relations with the U.S.S.R. Nor have recent efforts to apply formal conceptual frameworks to the stuff of Soviet foreign policy yet provided a convincing analyses of, say, Soviet policy in the Cuban missile crisis or the invasion of Czechoslovakia.

Given these difficulties, the best preparation for writing about Soviet foreign policy may be a coherent personal stance. It is such qualities that make the works of Louis Fischer or George Kennan valuable. It is also the outstanding virtue of Adam Ulam's *Expansion and Coexistence,* the work most nearly comparable to this one. Ulam consistently relates Soviet foreign policy to the political perspectives of particular leaders, and to domestic politics generally.

The major flaw in Wesson's work is the lack of any single unifying principle. Soviet foreign policy is discussed more in sequence than in perspective This difficulty is found both in the work overall and in particular sections such as Wesson's discussion of the origins of the Cold War. Wesson remarks in his preface that, ". . . . we are no nearer than ever to [understanding Soviet foreign policy.]. Perhaps it is not really possible [to do so.]." Since Wesson feels that our understanding of the Soviet political system as such as "at best tentative and superficial," his pessimism is understandable.

Of course, the author does analyze Soviet foreign policy repeatedly; he is not, after all, producing a chronicle. In the paragraph quoted above, Wesson tells how he intends to interpret Soviet foreign policy. (The Soviet response to the outside world may explain "the mainsprings of its behavior.") He will show how Soviet foreign policy has evolved in response to internal needs, ideology, the passage of time, and to problems of controlling the sometime Bloc. In practice, however, Wesson's approach is to select, seemingly at will, one or another possible explanatory factor. Nowhere in this book is there an explicit statement of the principle guiding this selection; one is left with the suspicion that selection is governed by convenience and "reasonableness."

There is no focused discussion of the role of leadership styles in relation to any one leader, or to a specific era of Soviet history. On page 96 we read, "How much was attributable to the personal character of Stalin no one can determine. Certainly, the period of his rule was marked by his personality." But his character does not, it seems provide a "large answer to the whys of Soviet foreign policy," because circumstances determined his rise. Sometimes Stalin is described as oriental, crafty, and ignorant of the outside world; sometimes he is called moderate and is given high marks for policy successes. His most ambiguous foreign policy venture, the pact with Hitler, is variously explained; the relationship of Stalin, as a policy-maker, to this policy is not made clear. It is hard to tell what Wesson thinks of the policy itself.

Wesson argues (page 157) that the experiences of World War II, especially those of fruitful relations with the West, affected the outlook of Soviet policy makers. Indeed, he suggests (pages 172–4) that the advent of less "leftist" American policy makers after Roosevelt's death helped to evoke uncooperative responses from formerly friendly Soviet officials. This analysis is not necessarily wrong, but one cannot identify the principle that leads Wesson to argue for a breakdown of mutual trust as a major explanation of Cold War origins.

Similarly Wesson's treatment of ideology is uncertain. It is never made clear whether ideology is a major factor in shaping policy; the author sometimes argues that ideology helped, sometimes that it hindered realization of Soviet aims. Moreover, Wesson does not clearly distinguish between consequences of Soviet policy that had contra-ideological or other undesirable effects, and consequences that were dysfunctional in terms of goals. Thus he demonstrates and decries the baleful effects of Leninism on the European labor movement. But did the splintering of European radicalism hamper Soviet foreign policy? Would the U.S.S.R. have more influence over French policy if the French Left were a united, radical, non-Bolshevik mass movement?

Here we come to a further problem of Wesson's analysis: despite his explicit statement that security (not defined) is the consistent goal of Soviet foreign policy, the book is not organized around the attainment (or lack of attainment) of this goal. If Soviet policy makers perceived security to lie in social transformations of the international environment, then the failures of world revolution —or even more, its frightening success in China— would indicate a focus on these policies. If security was thought of as a favorable military-weapons situation, then an analysis of Soviet foreign policy should concentrate on such questions.

All too often in this book, the merely spectacular displaces the important. How else can one explain the inordinate amount of space devoted to the Czechoslovak crisis of 1968–9? On Wesson's own showing, the military intervention in Prague was not a new departure in Soviet policy, but rather a defensive consolidation of the Soviet position. Yet Wesson spends some twenty pages on the crisis, while arms control and related issues get only scattered attention.

This concentration on Czechoslovakia is partially explained by Wesson's semi-explicit contention that here was an instance of Soviet foreign policy flowing from domestic (in this case, nationalities) policy. At issue here are not Wesson's accurate facts but rather the lack of systematic evaluation of the relation between domestic and foreign policy. The book contains many digressions on Soviet domestic politics: is their purpose information or explanation? Curiously enough, Wesson devotes very little space to the institutions and procedures of Soviet foreign policy, from the *Narkomindel,* to the Comecon, to the changing training of Soviet diplomats.

The strongest aspect of Wesson's book is his keen sense of the contingent nature of Soviet foreign policy, its frequently wasteful and confused policies, how much it resembles the policy of traditional states in small-minded and often fruitless cynicisms. There are also an admirable summary of Sino-Soviet relations, and a good exposition of the post-Krushchev policy toward the Third World. Of course, every reader willl have his own specific disagreement, such as the statement (page 180) that Stalin "had no real German policy" in 1944–5.

Wesson's work will give readers a competent, wide-ranging history of Soviet foreign policy. Will it also give them a heightened understanding of that policy? This reviewer prefers Ulam's provocative individuality to Wesson's cautious surrender to complexity.—HENRY KRISCH, *University of Connecticut.*

The Making of Canadian Foreign Policy. BY R. BARRY FARRELL. (Scarborough, Ontario: Prentice-Hall of Canada, Ltd., 1969. Pp. 181. $5.95 cloth, $3.50 paper.).

Professor Farrell's book can be judged neither by its cover nor by its title. Consequently, the scholar hoping for a book which discusses the dynamics of the Canadian foreign policy-making process and which contributes needed material for the burgeoning field of the comparative study of foreign policy is certain to be sadly disappointed. Instead, this "book is about Canada's diplomatic organization," and even then the prime "concern is the Department of External Affairs and the Foreign Service Officers who run that Department." (p. 2). The overall contribution of the volume, given its expressed purpose, will be discussed later, but, for the moment, an assessment of Professor Farrell's success in achieving the goals he set for himself seems to be in order.

No advantage seems to be gained by briefly reviewing the material covered by Professor Farrell except to say that he is mainly concerned with the organization of the Department of External Affairs and the Foreign Service Officer corps, the roles of the Prime Minister, the cabinet, the "under cabinet," and the Secretary of State for External Affairs, the structure of Parliament, and the nature of federal-provincial relations as these relate to Canadian foreign policy. It is adequate to state that the author has probably identified the appropriate governmental actors, even though the completeness of his description of them varies. For instance, it is interesting to note that nearly twice as many words are devoted to the place of women in the Department of External Affairs as is given to a description of External Affairs Officers, a topic which is dismissed in eighty-five words. (pp. 80–81). We have no quarrel with Professor Farrell's assessment of the importance of women, but the treatment of the External Affairs Officers seems to be rather incomplete.

Essentially, then, the author is concerned with the structure of the organizations charged with responsibility in foreign policy. In the process of leading us through this organization-chart-in-prose, Professor Farrell combines the type of material one would expect to find in a government manual, historical illustrations which sometimes replace rather than supplement analysis, and data on the ethnic, educational, and regional backgrounds of cabinet ministers, Foreign Service Officers, and Heads of Missions, as well as on the pertinent employment patterns in the department of External Affairs.

Unfortunately, though, while presenting the structure, the author has failed to discuss the dynamic relationships among and within the foreign policy participants he identifies; thus the book is essentially a skeletal organization chart. At one point, the observation is made that "all the heads of departments [in the cabinet] must agree on common foreign policy." (p. 20). However, there is no discussion of how decisions are reached in the cabinet or what happens if there are major disagreements. Similarly, it is not clear how various problems are brought before the cabinet for consideration. Later on it is stated that foreign policy officials are increasingly subject to "the thrusts of controversial opinion," (p. 140), but a discussion of Canadian public opinion on foreign policy matters, its origins, its transmission to policy-makers, its influence cannot be found. For the most part, the interesting material on the backgrounds of cabinet ministers and Foreign Service Officers remains merely interesting but not particularly enlightening since only a feeble attempt is made to relate the data to either the content of foreign policy or the nature of the foreign policy process. Referring to two tables, the author says of the Foreign Service Officers that "the consequent decline in morale becomes striking." (p. 107). However, evidence is not presented which indicates that morale is indeed declining, nor is there much discussion of the consequences of this trend. As a result, most of the data, while interesting, do not seem to be particularly relevant.

In summation, Professor Farrell has provided the outline of the governmental foreign policy organization, but he has not considered those customs, procedures, practices, and relationships which make the numerous institutions and individuals a dynamic problem-weighing, decision-making, and policy-implementing organization that is worth studying. Even the chapter entitled "Responsibility and Political Control" which discusses provincial-federal relations, the role of Parliament, and professional responsibility within the foreign policy bureaucracy is concerned mainly with structure and hardly at all with substance.

Up to this point, we have briefly commented on Professor Farrell's success in accomplishing the goals he set for himself, but now we shall turn to an assessment of the author's conceptualization of the book and its contribution to the literature on foreign policy. In the last chapter, prior to a presentation of nine suggestions or predictions about the conduct and content of Canadian foreign relations, Professor Farrell says, "Canadian foreign policy and diplomacy are shaped by many other factors beyond the diplomatic organization." (p.

166). He declines to discuss these "other factors" in the present volume, yet a majority of the author's predictions about Canadian foreign relations are based on unidentified and unexplained assumptions about the importance of these "other factors." It is difficult to discuss adequately anything more than the structure of the organization without considering these other variables, and certainly one's area of analysis must encompass such factors if the aim is to understand the making of foreign policy.

We have already mentioned the neglect of public opinion by Professor Farrell, and we must add to this the existence of only a slight reference to the role of interest groups and the failure to consider the influence of Canadian political parties and the general dynamics of Canadian politics. These and many other factors may be of varying importance in the foreign policy-making process, but at least their impact must be assessed. In addition to the numerous non-governmental factors, a book on the formulation of Canadian foreign policy should discuss in some depth the role of other departments of government. The foreign relations activities of the Department of Industry, Trade and Commerce and the Department of National Defense were purposefully neglected by the author with the justification that he wished to concentrate "on the development and implementation of Canada's political foreign policy," (p. 2); it is strongly suggested that the activities of these other departments also influence "political foreign policy."

Many of the problems mentioned heretofore might have been avoided had Professor Farrell provided some kind of conceptual framework to organize the subject. This failure is particularly disturbing since Professor Farrell edited the compilation, *Approaches to Comparative and International Politics*, which has stimulated so much of the recent activity in the comparative study of foreign policy. It is a pity that *The Making of Canadian Foreign Policy* does not heed the challenges issued in this earlier collection.—DAVID H. BLAKE, *University of Pittsburgh.*

The Majority of One: Towards a Theory of Regional Compatibility. BY MINERVA M. ETZIONI. (Beverly Hills: Sage Publications, Inc., 1970. Pp. 238. $7.50.)

The main title of this study ("The Majority of One") alludes to the overwhelming predominance of the United States within the Organization of American States. Its substance is therefore appropriately enough concerned with the achievement of American policy interests within that organization and in particular the working out of an increasing American preference for use of the OAS, as against alternative use of the United Nations, in the handling of Latin American disputes which might legally and logically have been referred to either. The survey of these developments is competent and the tendencies which it sees are accurate.

In evaluating its usefulness as a descriptive account alone I am impelled to suggest only three perhaps not very important cautions: (1) Mrs. Etzioni's research is limited in its primary sources almost entirely to official UN (and, to a lesser extent, OAS) documentation plus published memoir materials of a few of the main participants in these events and thus her account does not get much beyond the legalistic description of what actually did happen with very little explanation of why it happened that way or at whose particular behest. (2) The author is, not at all unexpectedly, more or less predisposed against the very facts of U. S. predominance and preference for the OAS which she describes and her presentation of them will accordingly give more satisfaction to some students of approximately the same events than it will to others. (3) She offers no significant discussion of developments after the Dominican crisis of 1965 which makes her work more than usually outdated for a book of this publication date (1970). The historical-legal-descriptive account, nevertheless, is still a sound one. None of its findings or arguments would need to be essentially revised as a result of even complete "correction" of these relatively modest shortcomings.

The sub-title of the study is "Towards a Theory of Regional Compatibility" or, as chapter 1 labels it, "a new theory of international organization." This really expresses the *raison d'être* of the work, or at least of its publication. The Richard Falk preface is touting this aspect of the analysis in particular when it refers to the whole work at one point (p. 10) as "a pathbreaking book of great interest." But the author is not well served either by the grandiloquence of her introducer or by her own compulsion, for whatever reason, to graft onto a perfectly acceptable descriptive analysis a theoretical construct that is lacking in rigor and even in relevance to her own case.

First, as to rigor. The argument is that we should get away from the old dichotomy between "universal" and "regional" characteristics of international organizations and replace it with the concepts of "compatible" as compared to "incompatible" regionalism. These last are to be applied through separate consideration of the "jurisdictions" and "divisions of function" or competing (universal and regional) international organizations, the interactions between which are to be perceived as "upward" or "downward transfers." At best, this terminology gives new names to what we all know. Thus, if NATO is cited as an "incompatible" regional organization because it focuses "primarily on threats [to security] from outside the regional group" whereas the OAS is at least in part "compatible" since it is "prepared to confront aggression anywhere—even within [its] own membership" (p. 22), one wonders what has happened to the older, equally serviceable distinctions between collective defense and collective security, which say the same thing and which are being displaced by nothing better.

More significantly, the new theory does not much help to explain or predict. We might, for example, agree on the labeling of an "upward transfer" after we saw one, but what would this tell us about the reasons for international organizational behavior or the possibility of its being repeated, or not, in the future? The new theory in the end is simply and fuzzily normative, at least in nomenclature. If an action is recognizably "compatible" or "upward" (incidentally, these are both "good" words) in character it is to be either sought or cherished; if it is either "incompatible" or "downward" it is to be lamented. In the history of OAS-UN relationships which she describes Mrs. Etzioni has, not surprisingly, much to lament. She could have lamented just as persuasively without these labels.

Ultimately, this is precisely what she does. She is for the most part critical of United States policy, which analytical position, however, is not much illuminated by resort to the new theory and certainly does not depend on it. She is very disturbed, for instance, with the early "failure of the OAS to restore peace and to restrain United States activities" in the Dominican crisis of 1965 (p. 204), but this failure as such has little to do with whether that organization was behaving "incompatibly" at the time, which presumably it was, and derives instead from the quite commonplace inability of the weak to influence the strong in almost any institutional setting.

Finally, the new theory is not a general theory at all except in the most tenuous and tentative way. In a brief statement of "conclusions" (pp. 208–218) which includes much of the very little non-Latin American material that this book contains, there is only one reference to an OAU action, whose allegedly "incompatible" character is itself at least arguable; one other reference apiece to "incompatible" actions of both NATO and the Soviet bloc, which organizations were virtually 100% "incompatible" in nature to start with; and allusions to two events that "might well" or "very well" happen "in the future" in Africa and in the Near East and one prospective development in regard to China that would be "not impossible." This is slim evidence on which to argue a general "continuum [she means: tendency] towards regional incompatibility in the adoption of enforcement action" (p. 213). Even if the finding of such a trend is true, so to label it offers, once again, virtually nothing of either explanatory or predictive value. No very useful theoretical paths have been broken here.—Keith S. Petersen, *North Carolina State University.*

Dag Hammarskjold's United Nations. By Mark W. Zacher (New York: Columbia University Press, 1970. Pp. 295. $7.00.)

Professor Zacher examines in detail Dag Hammarskjold's conception of the United Nations and the role of the Secretary General within it. He is particularly concerned with Hammarskjold's conception of the organization's goals and bases of power and more importantly the strategies and tactics that the Secretary General assumed for himself and the United Nations. Strategies are defined as ". . . courses of action which are oriented to a particular structure of relationships in a political system and particular issues in that system," and tactics are defined as ". . . policies which indicate how certain actors should act in the processes of initiating, utilizing, and directing specific means of action." (p. 4)

Hammarskjold is portrayed as a liberal democrat, a man who viewed himself as a citizen of the world, and a man whose training in economics and Swedish governmental service led him to accept the perhaps naive notion that he was solely a civil servant and not a politician. Hammarskjold was to maintain from the time he became Secretary General until his death during the Congo crisis that he could be and was an impartial servant of international organization and law.

The Secretary General viewed the fundamental purposes of the United Nations through an extrapolation from the UN charter which sought ". . . the prevention of armed conflict through negotiation, the prohibition of the use of force 'save in the common interest,' equal economic opportunity, political equality, and the rule of law or justice." (p. 22) Hammarskjold felt that the United Nations had five bases of power from which to achieve the Charter's goals. First, the organization derived power from the responsibilities members were required to accept via the Charter and UN decisionmaking bodies. Second, an emerging United Nations gained power through an impartial, skilled, international civil service led by a unitary executive. Third, the total diplomatic facilities of the United Nations, both formal and informal, gave power and legitimacy to the organization. Fourth, the increasing interdependency of nations in the world coupled with the fact that most of these nations were UN members increased the probability of power acquisition. Finally, the United Nations gained power from its capacity to provide technical and economic assistance to member nations.

Zacher, in the bulk of the volume, provides a propositional inventory of strategies and tactics derived from Hammarskjold's behavior and statements. These strategies and tactics are discussed in the context of the peaceful settlement of disputes, the control of the use of force, the promotion of arms control and disarmament, and the attempt to build a peaceful world order.

In the promotion of peaceful settlements of disputes Hammarskjold was primarily interested in keeping the great powers out of non-bloc disputes, a strategy he called preventive diplomacy. Tactically the role of the Secretary General in settling disputes should involve, if possible, private diplomacy rather than open public debate, and a conciliatory public posture that never takes partisan stands. The Secretary General, Hammarskjold contended, should play a mediating role only when such a role is not opposed by the great powers, or parties to a dispute. In seeking peaceful settlement of disputes Hammarskjold was sensitive to the probabilities of success and avoided United Nations participation in disputes that could not be settled, and hence would impair the legitimacy of the organization or any of its agencies.

As to controlling the use of force, Hammarskjold was opposed to involving the UN in the maintenance of order among bloc nations or in conflicts between great powers except when such disputes were so critical to world peace that a nonpartisan agency was the only hope for tension-reduction. Peacekeeping forces, as in the Middle East and the Congo, were manifestations of Hammarskjold's desire to return situations to the existing status quo and substitute or preempt external forces for UN forces. As with the settlement of disputes, the role of UN forces should be nonpartisan, low-keyed, and their use should be tied to great power and disputing party acceptance of them.

Hammarskjold's strategies for promoting arms control and disarmament negotiations entailed bringing the scientific communities from the Soviet and Western blocs together and attempting to create a United Nations framework for negotiations that incorporated participation by nonaligned nations and the Secretary General. Tactically, Hammarskjold tried to involve the United Nations in all phases of such negotiations, opposing schemes to exclude the UN and third world nations.

Finally, for the promotion of a peaceful world order, Hammarskjold contended that such an order was tied to political and economic equality; that the UN could be the site for national participation in world policymaking on an equal basis and that to provide the economic component of stability, the UN must provide economic assistance to third world nations. Each tactic that increased United Nations participation and hence its legitimacy was a step toward a peaceful world. Self-determination, law, political equality, economic development, were all prerequisites for peace and therefore necessary United Nations tasks.

Professor Zacher's analysis is informative and systematic, some fifty principles of strategy and tactics are abstracted from Dag Hammarskjold's behavior and statements as Secretary General. This study will provide useful analytical and historical material for beginning and advanced students of the United Nations. However, what remains troubling to this reviewer, given the continued escalation of the Vietnamese War, constant tensions in the Middle East, and big power economic and political involvement in the third world, is what remains to be written.

We need comparative data on Htmmarskjold's and U Thant's goals, strategies, and tactics, and some feel for the relative successes of the two Secretary Generals. We must critically assess the role of the United Nations in the international system of the 1960's and predict and prescribe policies for the United Nations of the 1970's. Lastly, and most importantly, we must begin to think speculatively and imaginatively about possible alternative international and national institutions designed to increase personal participation in decisions that affect one's life and about institutions that redistribute wealth, provide social services, and increase personal freedom while reducing cross-cultural violence. What place, if any, does Dag Hammarskjold's United Nations have, given the goals suggested?—Harry R. Targ, *Purdue University.*

Partners in Development, An Analysis of AID-University Relations 1950–1966. By John M. Richardson, Jr. (East Lansing: Michigan State University Press, 1969. Pp. 272. $8.50.)

This book exhibits many of the characteristic merits and defects of empirical academic research on government programs and policies. Least characteristic and most meritorious is the $10,000 cost of three years work including over forty interviews and an examination of the files of thirty university overseas projects. The resultant account of the sad story of AID-university relations is careful, reasonable, and fair. Less creditable are its unmitigated lifelessness and the dutiful genuflections to general "theory" which will surely earn for our social science a place in intellectual history beside that of medieval scholasticism.

The main, and to my mind best, part of the book presents a largely historical review of AID-university relations during six periods which Richardson identifies with the term of various agency directors or non-directors:

1. The Period of Genesis (1949–53), in which

university contracts—several originating in personal friendships of university and foreign officials—became embroiled in the conflict between the Mutual Security Administration emphasis upon strategic considerations and the Technical Cooperation Administration's "do good" philosophy.

2. The Period of Proliferation (1953–55), associated with the directorship of Harold Stassen and the establishment of the Foreign Operations Administration, during which the number of institutions involved in overseas projects rose from seven to over sixty.

3. The Period of Retrenchment (1955–57) under economizer John Hollister, director of the FOA successor International Cooperation Administration.

4. The Period of Inertia (1957–61) or leaderlessness under four different directors, in which staff, in effect, ran the agency for better and, often, worse.

5. The Interregnum (1961–62) of Fowler Hamilton's Agency for International Development.

6. The Period of Harmony (1963–66) under David Bell, best of the agency administrators, when many new contracts were initiated and a sense of rapport developed between agency and university representatives.

However, Robinson rightly distinguishes the off-again on-again harmony between agency and university brass from the more consistent hostility, cross-purposes, and red-tape that have marred relations between operating personnel in the field, on campus, and in Washington.

Other agencies, such as State and Justice, have remained remarkably oblivious of academia; but it cannot be said that any other agency, not even the Arms Control and Disarmament Agency, has contracted with universities for so long so large and important a program the quality of whose administration has oscillated so consistently between the pedestrian, the poor, and the atrocious.

Why this should be so is of both practical and theoretical interest, and Richardson attempts both a practical and theoretical explanation.

On the practical side, he observes that the university program has been a small frog in a big government pond; that many significant determinants of agency-university relationships have been uncontrollable by either party; that university contracts have been handled like procurement contracts with industry; and that agency personnel have been more concerned to enforce regulations than to achieve program objectives. It has been difficult for the agency to give universities the degree of independence necessary for the successful conduct of their work while retaining sufficient responsibility to protect both the government's interests overseas and the agency's interests at home.

His theoretical explanation is less satisfactory not because it is in any way unreasonable but because it is in every way insufficient, is subjected to no empirical test, and is compared with no other equally reasonable explanation of the facts at hand. We are told that the broad goals of an agency are not always "complementary," that they may be served by concrete measures whose actual effects on the goals are not always measurable, and that "Often the complementarity between the organizational maintenance goals of the Agency and the university-contract program has been very low." Indeed. We are also treated to a "matrix of relationships" and several sets of symbolic axioms.

A glossary with the full name and life span of the many initials under which foreign aid agencies have been incarnated would have been helpful.—Harold Orlans, *The Brookings Institution.*

Vietnam and China, 1938–1954. By King C. Chen. (Princeton: Princeton University Press, 1969. Pp. 436. $12.50.)

When Lyndon Johnson said in 1961 that "help" for the South Vietnam government was necessary because the enemy would have to be met in San Francisco if it was not stopped in Southeast Asia, he could not have had the Vietnamese Communists in mind as the main enemy. Nor (if he was in possession of the facts) could he have been referring to the Soviet Union, which had generally maintained what we now call a "low profile" in Southeast Asia, not to mention California. He could only have meant Communist China, which at the time was taking an active interest in foreign politics, at least in Asia. Unfortunately, the revival of the Vietnam war in the early 1960's occurred at a time when American understanding of Asian Communism was at a low ebb (partly as a result of the McCarthy-era purge of China specialists), and little was known about the relations between Peking and Hanoi. Since the United States entered the war, however, much excellent work has been done on the subject, including the book under review. (How regrettable it is that scholarship of this nature must be generated by conflict, when were it more timely it might prevent such conflict!)

King C. Chen, who has made extensive use of hitherto unavailable Chinese and French sources, sees the recent wars in Vietnam as but episodes in the two-thousand-year history of Sino-Vietnamese relations. He pictures the Vietnamese as willing absorbers of Chinese influence, whether it be Confucianism centuries ago, or Maoism today. (The final judgment here probably must await further study based on Vietnamese sources.) Nevertheless, the Vietnamese (of whatever political persuasion) are always wary because of China's occasional aggression against the exploitation of their country (most recently during the occupation of 1945–46). Thus, it is extremely unlikely that Chinese troops will ever be invited into Vietnam.

What emerges from the author's detailed account of relations between the various governments and political forces in the two countries during the Japanese occupation and revolutionary wars is a picture of extremely able and agile leaders, each of whom attends first to his conception of his own nation's interest, and then to partisan goals. Internationalist obligations are low-priority, if they are attended to at all. Here again there is historical continuity, for when China is weak or preoccupied (whether during the Sung Dynasty or the Great Proletarian Cultural Revolution), she has little taste for becoming involved in the affairs of nations to the south. But suspension of international cooperation does not mean suspension of ideological commitment, and there can be little doubt about Ho Chi Minh's unflagging commitment to Marxism. This applies to the period of the common resistance against Japan, and what Professor Chen considers to have been Ho's essentially dishonest "commitment" to the principles of Sun Yat-sen. Of this epoch in Sino-Vietnamese relations, Chen writes with considerable authority, having been in south China during World War II and interviewed Chinese leaders who were in personal contact with Vietnamese revolutionaries. However, Ho Chi Minh was a complicated man and Marxism is a complex ideology. It seem doubtful that Ho was being deliberately misrepresentative when he told the Chinese Nationalists that though he was a Communist, "Communism will not work in Vietnam in fifty years." (p. 82)

This study is more historical than political science. Nevertheless, it abounds in raw material for scholars in a number of disciplines. Perhaps some social scientist will be able to explain what Professor Chen observes but cannot explain—why America has passed up so many opportunities for peaceful settlement in Indochina.—JAMES D. SEYMOUR, *New York University*.

China's Turbulent Quest. BY HAROLD C. HINTON. (New York: The Macmillan Company, 1970. Pp. 340. $7.95.)

Professor Hinton of George Washington University published a major work on Communist China's foreign policy entitled *Communist China in World Politics* (Boston: Houghton Mifflin Company, pp. 527) in 1966, when he was still associated with the Institute of Defense Analysis. The book was considered as one of the most detailed, comprehensive, and well-documented studies of Communist China's foreign policy strategy and external behavior up to 1965, and had widely been circulated for a variety of teaching and reference purposes. The new volume under review is in essence an updated, but condensed, version of his 1966 book under a different title.

In the new book Hinton expands the analysis to cover a number of significant developments since 1965. Domestically, China has gone through an unprecedentedly dramatic political upheaval of the Cultural Revolution. The upheaval brought into the open the acute ideological and policy struggle between the "two lines," the Maoist and the Liuist. Externally, the Vietnam War and the Sino-Soviet dispute have escalated further to a level of no return and created great strains on Chinese foreign policy. On the other hand, the new book abridges the 1966 version and drops all the footnotes. Clearly the new book is intended for the general public and non-specialists who, Hinton thinks, would not care to be burdened with details and elaborate documentation. For those who are acquainted with his 1966 book, the structure, approach and conclusions of the new book are not unfamiliar.

Organized in three parts, the book first traces chronologically Communist China's policy lines and behavioral patterns: from the legacy of the Chinese Communist movement, the "armed struggle" of the early fifties, the peaceful coexistence of the Bandung era, the emergence of the Sino-Soviet dispute, the escalation of the Vietnam War, to the diplomacy of the Red Guards. The analysis focuses mainly on the conflicting orientations between the Maoist strategy of the "people's war" and the more moderate approach of "state diplomacy" favored by the non-Maoists. Shifting focus from the temporal to the spatial dimension, the second part discusses China's external relations with individual nations in various political and regional camps. A brief discussion of the structure and process of China's foreign policy making and implementation is also included. The first two parts are designed to shed light on China's foreign policy from the two different perspectives of temporal and geographical settings. Unfortunately, the two parts become awkwardly repetitive. The book closes with the author's overall assessment of China's failures and achievements and their implications for American foreign policy.

On his research strategy, Hinton stated in his 1966 book:

> I proceed on the basis of no general theory of social or political action. . . . I prefer history, in the sense of observed data and inferences from them, . . . I believe that what is sometimes called kremlinology, demonology, or the study of esoteric communication is essential to an understanding of Communist China. (p. viii)

In 1970, his methodological orientation remains unchanged. (p. x) His enthusiasm about the historical approach is clearly reflected in the extremely rich collection of detailed information and skillful use of source materials contained in the book. However, the book falls short of the standard of methodological and theoretical rigor that the contemporary social science analysis demands. It is unfortunate that Hinton does not utilize empirical theory to organize the massive data and impressive factual knowledge he possesses. Nor does he develop hypotheses for systematic analysis and conceptualization. On the whole, the book is descriptive, rather than analytical and explanatory.

Hinton's reliance on the "kremlinologist" approach has led him to dwell extensively on such factors as the ideology, personality, and motivation of key political actors, rather than on the more definable sociological variables used by the "objective-capability" analyst. As a result, his inferences are often derived from intuitive judgment with little empirical evidence and at times appear highly speculative (see pp. 43, 73, 137, 138, 206 and 286).

Hinton's conclusions suggest that Communist China is essentially not an "expansionist power" in the conventional sense of conducting aggressive war on foreign territories (p. 285). The leadership under Mao prefers the strategy of the "people's war" by encouraging "self-relient" revolutionary movements "from below" in the Third World with China's moral and material support. The rationale of the strategy is clear: it is "at modest risk and cost to Peking" (p. 253) and has proved effective in countries besieged by political and social instability. Confronted with such a strategy, Hinton cautions the U.S. to avoid "excessive military intervention" (pp. 304–307). But he argues that the U. S. should never "withdraw precipitately." (p. 307) Basically Hinton subscribes to the concept of "military containment" as a key instrument of American policy.

Regardless of its usefulness under certain conditions after World War II, the strategy is essentially a "negative" response to challenge from the left and based on fear of political change. A regime that requires military protection from without when confronted by internal insurgency is probably one whose political viability cannot be saved. Indeed, if China's basic strategy is the "people's war," as Hinton points out, and as Mao recently reaffirmed in one of his rare important statements issued on May 20, 1970, then the validity of "military containment" should be reexamined. The development of the Vietnam conflict and the ineffective American military intervention serves a clear case in point.

In this respect it is rather disturbing to note that Hinton criticizes the failures of the U.S. to make the invasion of North Vietnam a "credible threat" (p. 303) or to actually "invade just north of the 17th parallel" (p. 304) as a "basic strategic mistake." He assures the reader that "an American invasion of North Vietnam need [not] lead to a Sino-American War." (p. 304) He would now probably find the American intervention in Cambodia a sound strategic move. Moral and intellectual vulnerability of such arguments notwithstanding, it is doubtful that the Korean experience and the pattern of the expansion of war in Indochina would substantiate his arguments.

A rather common shortcoming in foreign policy making and analysis alike has always been the inability of the policy-maker and analyst to perceive and appreciate the adversary's viewpoint. Hinton seems to be no exception. His conclusions suggest that much of Communist China's international isolation and failures are due to Peking's own behavior" (p. 285) and are therefore "self-inflicted" (p. 286). Clearly, a balanced evaluation of China's hostility and belligerence toward the West, and toward the U.S. in particular, should take into account the initiatives taken by the U.S. in building up the military containment that China finds provocative and threatening. In the eyes of the Chinese, it is the Americans who have fought and are still fighting at China's doorsteps and have organized a series of bilateral and multilateral alliances to encircle China. Although Hinton is now prepared to go along with some conciliatory steps on the trade of nonstrategic goods and China's admission to the United Nations, the reader may wonder how far such steps would go while the U.S. continues to maintain its basic miltary and political posture around the peripheries of China.—YING-MAO KAU, *University of Illinois.*

Legal Effects of United Nations Resolutions. BY JORGE CASTAÑEDA. (New York: Columbia University Press, 1970. Pp. 243. $10.00.)

Several "musts" characterize the modern international lawyer. First, he must be in tune with the forces of the world's social complex, for these forces give reality to the direction and depth of legal change. Thereafter, he must be particularly attentive to the social ends of the world community so that his influence on public decision makers will cause them to give due attention to relevant forces and values. Moreover, he must be persuaded that a world's legal order consists of a process and is not simply a condition.

The world legal community, restricted to a more primitive and more uncoordinated life-style than that enjoyed by many national States, has a fundamental interest in the identification and development of its processes. This central concern is also reflected in analytical inquires into the "sources" of such community law. One listing is to be found in Article 38 of the Statute of the World Court. However, with the emergence of a myriad of important and seemingly permanent international organizations, there has been an increased recognition that within such forums there have been new processes at work. These have such forums there have been new processes at work. These have been producing new principles, standards, and rules of law for a constantly changing community. Moreover, the emerging community has been an augmented one consisting of States, international organizations, and individuals claiming in their own right the benefits of a dynamic jural order.

In this book, Dr. Castañeda, a distinguished Mexican international lawyer and foreign office official, has examined a fundamental law creating procedure by the organs of the United Nations, particularly the General Assembly. He has also analyzed comparable procedures on the part of other important international entities. He has demonstrated that the nonrecommendatory resolution of such organs is a constitutive legal process, and has identified the areas within which such resolutions (also known variously in practice as "declarations," "recommendations," "agreements," "pronouncements," as well as by other terms) have mandatory operative effect. The author gives much attention to those areas and situations where such an effect may reasonably be believed to exist.

Such areas are two: internal to the organization, and external. The latter deals, according to his structure, with (1) international peace and security, (2) the existence of facts or concrete legal situations, (3) a situation where the binding force rests on instruments other than the Charter, (4) those which express and register agreement among the members of an organ, and (5) those which contain declarations or other pronouncements of a general nature. The author admits that this classification is not without its difficulties and complexities. They should be regarded as an initial classification, nothing more. Nonetheless, the formulation of such a scheme is a major contribution.

The traditional view of law is that it should be complied with—more or less, and always preferably more. The author makes it clear that resolutions of international organs, if they are to be law, must be complied with. Considering their reception by the community, can they be law? Most assuredly. And, in suitable situations, are they law? Most certainly, and to the same extent that formal treaties constitute a process and source of international law.

In both instances, the resolution and the treaty, the position of the so-called sovereign nation-state must be taken into account in assessing the jural quality of the prescriptive event. However, in measuring the role of the State, it must be noted that the formal organization of an international institution, such as the UN, is in more instances than not a great deal more important and more impressive from a political point of view, than a vast proportion of its individual members; that the member States which have given adherence to the adopted resolutions are more important and more impressive than those which have voted in opposition or have abstained; that even the imperfect mandate of sovereignty has been much reduced in importance because of the elimination of the unanimity rule in international organizations (although in some instances there is the competing concept of "consensus"); and, also because of the increased acceptance of the view that it is the function of customary international law (produced by non-states or by fewer than the totality of States in the community) to bind States, even against their will.

Also, and of vast importance, it has come to be recognized that the Charter of the UN is a constitutional document. This has meant that one can rely on the legal doctrine of effectiveness of the institution to support its operations when they follow the spirit or general purpose of the organization. In this manner valid action can be taken on the basis of the powers implied from the constitutional prescriptions which are either granted or are not denied by the constitution. Where such powers have been perfected by practice which has received community approval, the resulting constitutional "gloss" provides doctrinal support for the law of such resolutions.

Additionally, the author takes full account of such factors as the intent of the drafters of the constitutional documents, the language selected including logical interpretations of such language, and the practice of the organs and the constituent States. Because of his concern for the mandatory or binding quality of resolutions, he examines them as "legislative" or law-creating instruments. It is his view that they can serve such a function as well as putting preexisting customary international law into a formal and identifiable written form.

It has long been the position of many international lawyers that a sympathetic attitude ought to be taken to the subject of proof of the existence of customary international law, since it does not literally speak for itself. It might be urged with equal conviction that a sympathetic attitude ought also be taken toward the question of whether a given resolution in a specific situation sets forth a principle, standard, or rule of international law. Why, indeed, should the *form* which surrounds a General Assembly resolution when unanimous or nearly so, be thought to be relevant to the binding force of the norm which it signifies? Should not the vote in its favor, with the seemingly non-rebutable presumption that the position reflected in the vote is entitled to respect and will be respected, have the same significance in the formation of a binding duty as a duly subscribed and ratified treaty? Such resolutions can give us the base for a modern if not "instant" international law at a time when the treaty—with its ancient appurtenances and appendages—has not always kept pace with world needs.

The resolution, like some swiftly moving customary international law of the space age, constitutes in suitable cases a process for the ready identification and promulgation of international legal norms. Dr. Castañeda has shown that logic and experience indicate that the resolution serves this function.—Carl Q. Christol, *University of Southern California*.

Conflict and Communication: The Use of Controlled Communication in International Relations. By John W. Burton. (New York: The Free Press, 1969. Pp. 246. $7.95.)

"Controlled communication" refers to the participation of scholars in meetings that take place between representatives of nations involved in a conflict. The technique is based primarily on the assumption that conflict is subjective and therefore avoidable or resolvable if perceptions of the parties in conflict can be altered, different interpretations of behavior and assessments of values and costs can be offered, and new options introduced into the discussion. The task of the scholars is *not* to suggest specific *substantive* interpretations or solutions to problems, as might be done in more conventional discussions which include third party participation, but rather to interject *theoretical* observations about the nature of conflict. In the author's words, "The strategies of control seem to be those of the clinical psychoanalyst, the case-worker, the industrial psychologist and the family counsellor" (68).

The purpose of the book is to give a report on the usefulness of this controlled communication technique "as a means both of the analytical study of world society and of conflict resolution and avoidance" (xii). In the first part of the book, the author outlines the procedures for setting up the discussions; the difficulties involved in determining parties to a conflict, gaining permission for the foreign representatives already assigned to London to engage in the academic experiment, and then establishing and controlling the communication once the discussions were launched; and, finally, some hypotheses and insights apparently generated by the discussions.

In the second part of the book, the author makes an assessment of the significance of controlled communication. He describes this technique as the logical extension of two distinct trends. The trend in the analysis of interstate politics progresses from "broad generalizations and synoptic accounts of history to micro-studies of the behavior of states and processes that enable the observer to come closer to the subject matter" (173). Simulation and content analysis are recent developments of analytical techniques designed to examine these latter, more narrowly focused processes; and controlled communication, consisting of features common to both of these techniques, represents a logical extension of these developments.

The second trend concerns the role that is proposed for third parties in the peaceful settlement of disputes. The author argues that "there has also been a progression in dealing with conflict from direct authoritarian judgments" (such as court rulings) to those of decreasing involvement of the third party, such as arbitration, conciliation, mediation, and good offices, in that order. The reason for this trend is that experience in peace-keeping in this century has demonstrated that "it is only the parties involved that are in a position to resolve conflicts, and that external coercion has a counter-productive effect" (170). Controlled communication, therefore, is another step in the direction away from third-party involvement and toward self-imposed solutions, since in situations of controlled communication the third party members (the scholars) do not deal with the substantive issues, but rather the theoretical context of the conflict.

Given this unique place of controlled communication as an extension of the two trends just mentioned, "controlled communication thus provides a meeting point of analysis and resolution of conflict" (174).

Overall the book is quite stimulating, providing some fresh insights into the development both of the analytical study of international politics and the roles of third parties in negotiation. Furthermore, the author makes a persuasive argument that controlled communication may prove to be an important addition to the ways by which third-party intervention in negotiations contributes to the resolution or avoidance of conflict. The only evidence that controlled communication did, in fact, make such a contribution, however, are the assurances of the author. And he is quite cautious: "All that can be said is that communication was established where previously it did not exist, perception and attitudes of participants altered during the exercise, and events followed, some of which could have been directly or indirectly related, but which could have been due no less to many other influences that were operating on the parties simultaneously" (217).

The lack of detailed comment about what occurred during the discussions, as exemplified in the above quotation, leads to the principal objection of both the book and the technique: the severe restraints imposed by the need for secrecy. As the author admits, "Parties engaged in violent conflicts cannot be expected to respond to a request to meet, even for academic purposes, unless not only what transpires but even the fact of meeting is secret. The secrecy aspect inhibits discussion among colleagues, injects into academic life an unwelcome element, and imposes some restraints upon publication of research papers" (232–33). Unfortunately, he then dismisses these objections all too easily: "However, this is no new experience; it is becoming more and more familiar as interaction between government and scholar develops. There are means of generalization after many separate analyses" (233).

The author apparently misses the crux of the objection to secrecy when he implies that because it is not new, its limitations have somehow been circumvented. The whole thrust of the behavioral movement, with which the author seems to identify so strongly, indeed the whole thrust of the social sciences in recent years, has been to make open to group scrutiny the substance of the data and the procedures of data analysis that form the basis of generalization. A scientist's work, his entire research cycle, must be subject to evaluation and replication if consensus on the findings is to be reached. Even speculative findings, which make up much of the theoretical observations in Professor Burton's work, suffer substantially when the conditions or events which stimulate the observations cannot be revealed. Perhaps fifty (?) years from now, when the data and detailed analyses *are* revealed, the work will be considered quite valuable. But, in the meantime, as Scott Greer argues in *The Logic of Social Inquiry* (Aldine, 1969), "outside the pressure cooker of policy-making, outside the nation state, is a larger scientific community which must be shown—not harangued" (8).

Despite these objections, the book is well worth reading. It reports on a new and potentially important technique of conflict control, a technique which the author convincingly argues deserves further study and experimental application. The limitations of controlled communication, as an analytical method for the study of world politics, should not detract from the book's original contribution to the literature on the role of third parties in the peaceful settlement of disputes.—David W. Moore, *United States Military Academy.*

Development Assistance in the Seventies: Alternatives for the United States. By Robert E. Asher. (Washington: The Brookings Institution, 1970. Pp. 235. $6.95.)

The United States foreign assistance effort continues to be treated as an annual and emergency one. It is also a steadfastly American undertaking, largely planned and implemented by a national aid establishment under the close scrutiny of Congress. The program's primary mission has never been developmental; it has been more consistently designed and rationalized as a means of supporting the viability of friendly states against invasion and insurrectionary violence. If early post-war conditions made this type of approach at least understandable, present circumstances make it untenable. The current fundamental and world-wide revolutionary situation is as threatening to world order as it is unyielding to cold war diplomatic styles.

In a new work, Robert Asher, a well known student of development assistance, argues for an enhanced and restructured American aid effort, one which is conditioned by the responsibilities of affluence and the new parameters of the international system. Many of the proposals which he offers are quite familiar; some were being debated more than a decade ago. But combined they suggest a new and substantial departure from both the organization and the substance of previous endeavors, one which the author feels would serve to establish patterns of mutuality and self-help in the international development effort.

Asher sees as a primary need the reorganization of the U.S. development assistance enterprise—to give it a more multilateral and autonomous character. Multilateralization would be accomplished by a shift of the major portion of the nation's capital assistance program to international agencies and by systematic efforts to increase the reliance on recipient nations and international advisory groups for planning and review of the remaining bilateral undertakings. Even the responsibility for in-country programming and performance monitoring operations would increasingly devolve to multi-national agencies, with a subsequent reduction in the number of U.S. administrative personnel in the field. American technical specialists would begin to be hired directly by recipient nations with U.S. technical assistance agencies coming to serve primarily a brokerage function.

Related structural reforms would be directed at increasing the autonomy of American bilateral aid institutions. The fundamental modification would involve replacement of the Agency for International Development with three essentially independent agencies: a development cooperation bank, a technical cooperation foundation and a private investment corporation. The new agencies would have considerable discretion, receiving from Congress a general rather than a detailed mandate and necessary appropriations on a multiyear basis. They would also enjoy substantial flexibility in seeking additional sources of finance independent of the appropriations' process. Their operations would be kept distinct in every way possible from military assistance programming.

Finally, Asher proposes numerous substantive changes. They include a marked increase in the volume of new aid (with the now familiar 1% of G.N.P. offered as a target figure), efforts to ease the terms of trade and debt-servicing obligations of low-income states, and reductions both in the lending rate and in restrictions imposed on the recipient nation's utilization of aid resources. He sees these substantive reforms, particularly the latter ones, as primary means for operationalizing the concept of self help.

Together the proposals suggest his central assumptions: The U.S., through a systematic de-Americanization of responsibility for its aid program and the provision of meaningfully increased resources, could serve to create the type of responsible and development-specific bargaining relationships between rich and poor states which would facilitate both the more effective utilization of aid resources and economic growth itself.

There is much which is unexceptionable in Asher's assumptions and proposals, certainly within the limits imposed by the range of subjects he chose to cover. Some omissions, however, might be mentioned. For instance, there is the question of the capacity of international development assistance institutions to carry the burdens the author's reforms would impose on them. Decentralization of responsibility by itself would hardly serve to order the relationship among the multitude of existing agencies with their differing specializations, constituencies, experiences and development models. Their past difficulties with the coordination of effort testifies to the problem. But Asher has little to say on the matter of establishing priorities and roles despite its clear relationship to the effective implementation of his reforms. Perhaps he felt such a consideration would have taken him too far afield. The reader may note, however, indications at other points of his willingness to be discursive—in numerous brief asides on matters ranging from population problems to protectionist sentiment in the fisheries industry.

A more serious omission, I think, is his failure to translate an awareness of the political dimensions of development into proposals for change. He discusses at several points the critical importance of a nation's leadership and institutional capacities in determining the rate and stability with which growth occurs; he alludes to the well recognized fact that aid can be uncorrelated or even negatively related to institutionalization and economic growth; he refers to the insensitivity toward political and social aspects of development which has characterized both multilateral and bilateral assistance programs. But this sensitivity is not reflected in either the substantive or organizational changes he advances. He fails to convey in them the need or the means for introducing political criteria systematically into development assistance programming. There is, for instance, no major proposal of the type recently advanced by the Presidential Task Force on International Development—for the creation of a U.S. international development institute to have as one of its primary tasks research on the problems of "social and civic development."

These observations aside, Dr. Asher's effort is a worthy one. As a balanced mixture of undogmatic advocacy and careful analysis it highlights critical issues and provides a sensible assessment of American opportunities and responsibilities in the area of aid and development. Times, of course, are scarcely propitious for the reforms as such. Asher himself displays few illusions, especially about the current prospects for those core proposals which would increase the amount of assistance while reducing Congressional controls over the commitment of resources. It should be noted, however, that his work has apparently found a singularly receptive audience in the International Development Task Force. Certainly there are repeated and striking similarities between Asher's major recommendations and those which the Task Force has more recently advanced.—ROBERT W. HUNT, *Illinois State University*.

United Nations in Economic Development—Need For A New Strategy. BY SUDHIR SEN. (Dobbs Ferry: Oceana Publications, Inc., 1969. Pp. 351. $10.00.)

Mr. Sen has spent some ten years in technical assistance work at the United Nations. He is mightily disappointed at what he considers to be the lack of a creative and meaningful response by the U.N. "family" to the crisis of development. He lays the blame on poor management. An expert's answer to a problem that is quite likely much more profound than Mr. Sen's technical treatment implies.

The book is not a critical analysis of the whole range of the economic development programs of the U.N., but rather a terribly redundant account of the severe weaknesses of the Expanded Programme of Technical Assistance and of the subsequent U.N. Development Programme. The treatment is both general and specific.

The author argues that the weaknesses of the EPTA procedures flowed from the failure to follow the guidelines for the awarding of money adopted by the General Assembly. Rather than take a hard look at projects suggested, the governing body strives above all for consensus, takes the country requests at face value and balances the agency allocations so as to avoid friction. Referring to the Expanded Programme, Sen charges, "Its bad procedures have produced bad programmes and poor leadership. They have been more conducive to bureaucratic growth than to economic growth." (306).

All efforts to right the situation seem to have failed. Even the merger of the EPTA and the Special Fund did not result in new standarized, effective procedures. Instead, "The entrenched no-changers, through adroit manoeuvers and capitalizing on the multiple rifts in the U.N. system, won an outstanding victory." (141).

One wonders why it is so. Certainly not regard for national sovereignty. Perhaps it is the jealousy of the participating agencies. Mr. Sen seems to think so. It seems that the phenomenon of trying above all to protect what one has operates at the level of the U.N. as well as in Congress and city councils. It would seem that international experts in economic development management cannot rise above petty self-interest. Or perhaps the cause is related to the recently identified "Peter Principle", namely, "In a Hierarchy Every Employee Tends to Rise to His Level of Incompetence." (Laurence J. Peter and Raymond Hull, *The Peter Principle,* Bantam edition, 1969, p. 7).

In any event, what to do about it? Half the book is devoted to fairly extensive and specific recommendations for reform. The recommendations are essentially three in number.

(1) to merge the two components of the current UNDP and to reform internal procedures.
(2) to redesign the structure and procedures of the specialized agencies and to bring all development activities of the U.N. under one general manager.
(3) to institute at the country level a single representative to serve as a responsible, top-level development diplomat.

Following these structural changes, Sen argues that priorities could be dealt with intelligently and the potential of the U.N. could become actual accomplishments.

The approach Sen suggests is surely a sensible one. The reader is left wondering why the abuses have been allowed to continue. That question, the author does not really answer. Questions need to be raised about the real values and objectives of the men and organizations involved, the broad environment in which the technical assistance program goes on, and the politics of the many parties related to the development enterprise. From these broad perspectives one might get some clues to what does seem, on the face of it, to be a tragically absurd state of affairs.

This book is a ripping account of mismanagement from a man who has been on the inside. The book is badly organized and redundant. Some judicious pruning would have strengthened the force of the argument. Yet the proposal strikes this reviewer as a sensible one.—LOUIS F. BRAKEMAN, *Denison University.*

ERRATUM

The book review of *Les Groupes Communists Aux Assemblies Parliamentaires Italiennes (1958–1963) Et Françaises (1962–1967)* in the June *Review,* 4th paragraph, line 10, p. 649, should read "as *un*important as one might superficially assume."

ANNOUNCEMENTS

The sixty-seventh Annual Meeting of the Association will be held September 7–11, 1971, at the Conrad Hilton Hotel, Chicago, Illinois.

As of January 1, 1971 the Book Review Editor of this REVIEW will be Professor Philip Siegelman of San Francisco State University. All correspondence pertaining to book review matters should be sent to Professor Siegelman at the following address:

210 Barrows Hall
University of California, Berkeley
Berkeley, California 94720

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March, 1971

James W. Clarke and E. Lester Levine, Florida State University, "Marijuana Use, Social Deviance and Political Alienation: A Study of High School Youth"

M. Kent Jennings, University of Michigan, and Richard G. Niemi, University of Rochester, "The Division of Political Labor between Mothers and Fathers"

Chong Lim Kim, University of Iowa, "Socio-Economic Development and Political Democracy in Japanese Prefectures"

Gerald H. Kramer, Yale University, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964"

Frank Levy, University of California, Berkeley, and Edwin M. Truman, Yale University, "Toward a Rational Theory of Decentralization: Another View"

Duncan MacRae, Jr., University of Chicago, "Scientific Communication, Ethical Argument, and Public Policy"

Harvey C. Mansfield, Jr., Harvard University, "Hobbes and the Science of Indirect Government"

Theodore R. Marmor, University of Minnesota, "Income Maintenance Alternatives: Concepts, Criteria, and Program Comparisons"

Robert Melson, Michigan State University, "Ideology and Inconsistency: The Politics of the 'Cross-Pressured' Nigerian Worker"

Adam Przeworski and Glaucio A. D. Soares, Washington University, St. Louis, "Theories in Search of a Curve: A Contextual Interpretation of Left Vote"

Douglas W. Rae, Yale University, "The Democratic Guarantee: An Index and Some of Its Analytic Arguments"

Duff Spafford, University of Saskatchewan, "A Note on the 'Equilibrium' Division of the Vote"

Michael Taylor, University of Essex, "Party Systems and Governmental Instability"

Donald S. Zagoria, Hunter College, City University of New York, " 'Rice' and 'Feudal' Communism in India"

June, 1971

Blair Campbell, University of California, Los Angeles, "Prescription and Description in Political Thought: The Case for Hobbes"

James A. Caporaso and Alan Pelowski, Northwestern University, "The European Economic Community as Time-Series Quasi-Experiments"

Daniel W. Fleitas, Florida State University, "Bandwagon and Underdog Effects in Minimal Information Elections"

John E. Jackson, Harvard University, "Senate Roll Call Voting: Statistical Models"

Arend Lijphart, University of Leiden, "Comparative Politics and the Comparative Method"

John E. Mueller, University of Rochester, "Trends in Popular Support for the Wars in Korea and Vietnam"

David E. RePass, University of Minnesota, "Issue Salience and Party Choice"

Paul M. Sniderman, Stanford University, and Jack Citrin, University of California, Berkeley, "Self-Esteem and Isolationist Attitudes: Psychological Sources of Political Beliefs"

Sidney G. Tarrow, Yale University, "The Urban-Rural Cleavage in Voter Involvement: The Case of France"

Jack E. Vincent, Florida Atlantic University, "Predicting Voting Patterns in the General Assembly"

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William J. Crotty, Northwestern University, "Party Effort and Its Impact on the Vote"

Allan Kornberg and Robert C. Frasure, Duke University, "Policy Differences in British Parliamentary Parties"

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* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

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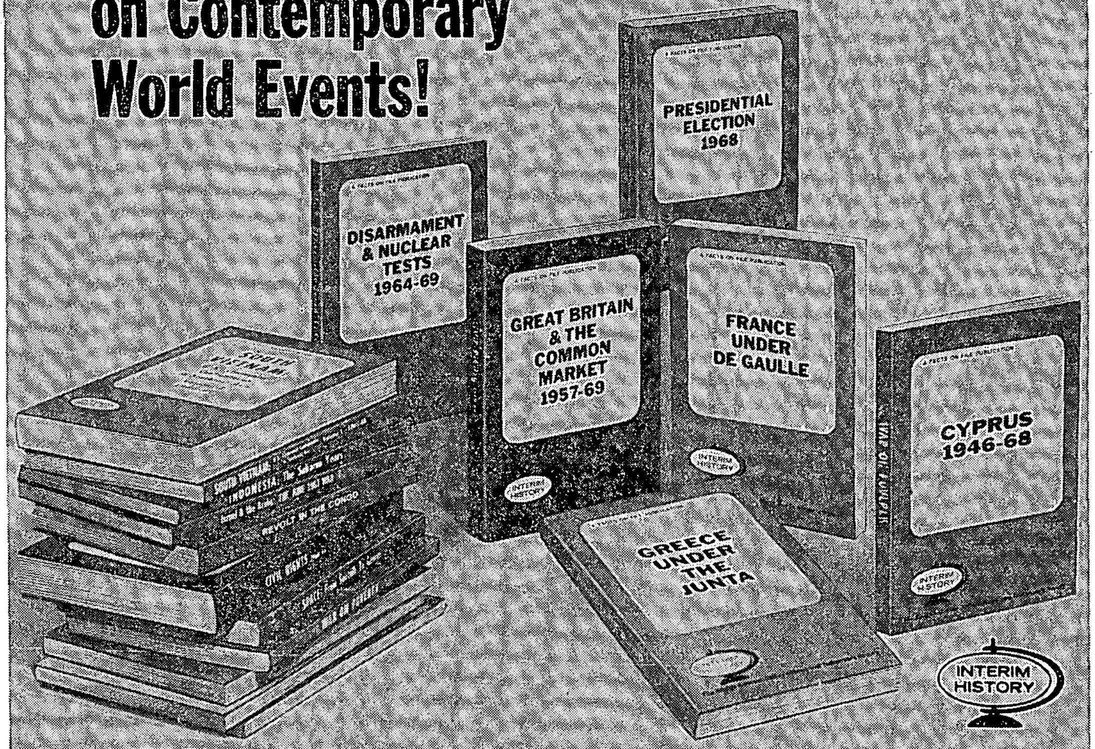
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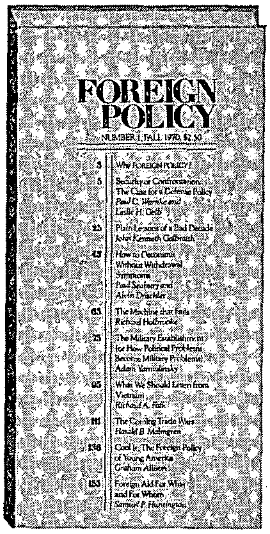

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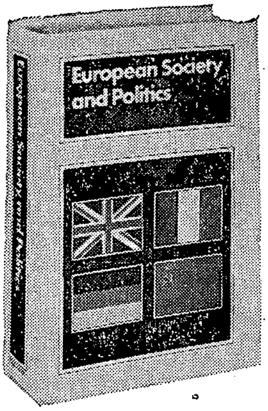

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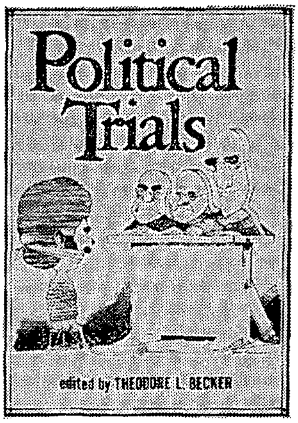

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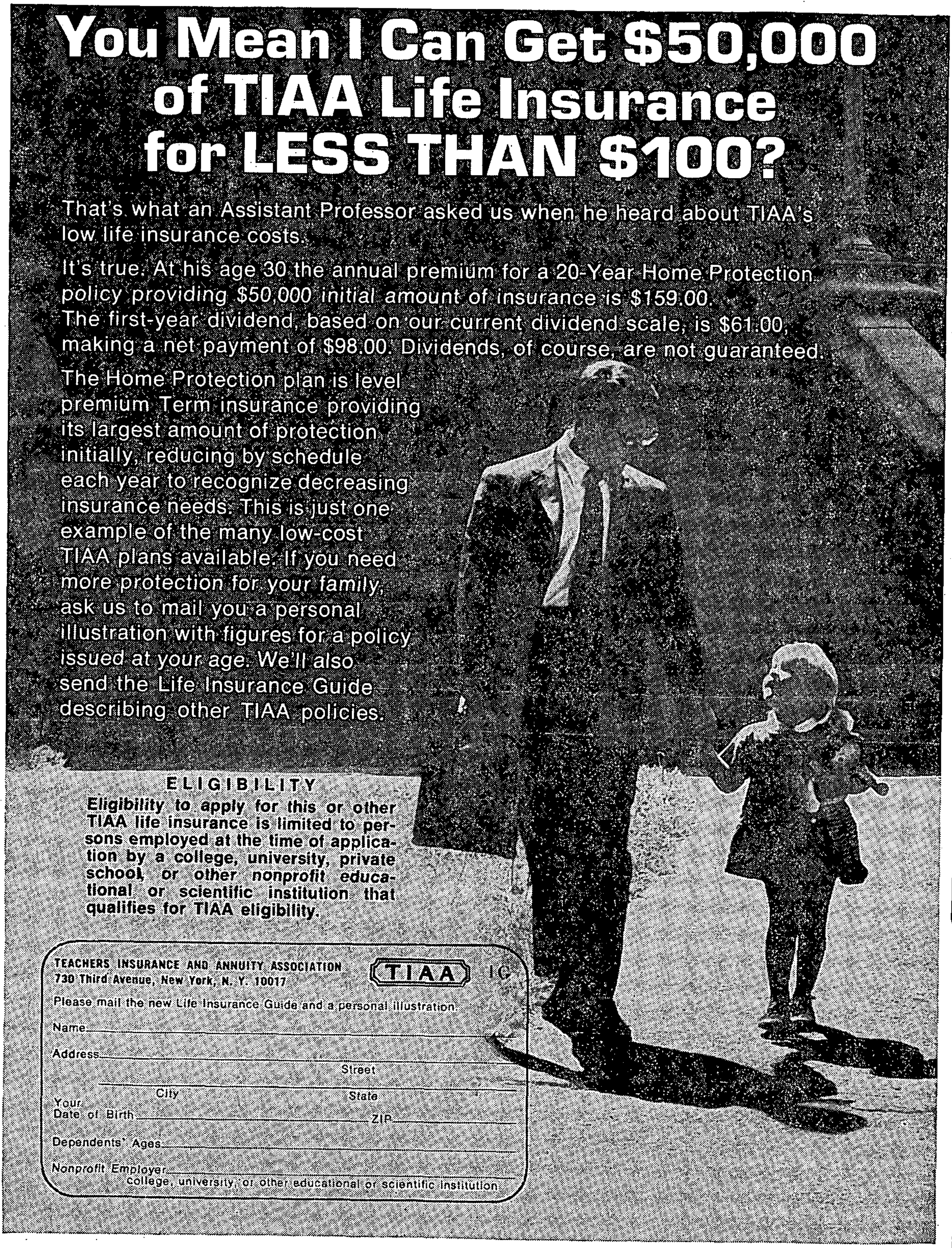

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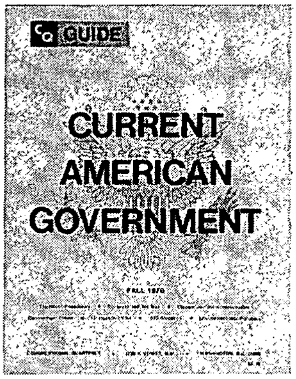

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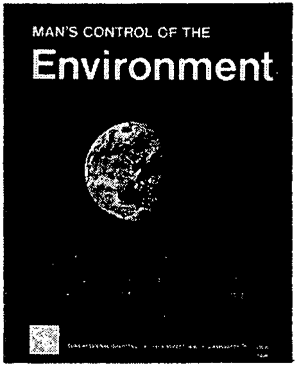

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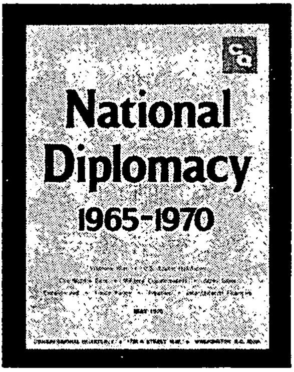

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